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PREFACE

As everyone knows who has tried it, the preparation of a review of literature covering a five- or ten-year period for the *Psychological Bulletin* or *Physiological Reviews* is patience-demanding and laborious. But according to the authors of the *Annual Review of Psychology* this task is much less difficult than integrating the digests of psychological literature for a single year with basic foundation material from the recent past.

Perhaps the Editorial Committee is responsible for some of the difficulties that confront the authors, for it divides the general field of psychology into segments so as to produce from 18 to 20 chapters of suitable length for a book of approximately 450 pages. This type of subdivision cannot be done without bringing together in certain chapters some topical materials that are only remotely related. For example, somesthesia, algesia, olfaction, and the common chemical sense actually have little in common. Likewise, the major part of Comparative Psychology and Physiological Psychology are not closely integrated, whether one considers advanced specialists or psychologists who themselves are not specialists in these fields.

What steps, if any, has the Editorial Committee taken to unify the subject matter of the different chapters? Even while planning Volume 1 it began to consider the separate merits and demerits of presenting two chapters (Diagnostics and Therapy) under the general heading of Counseling Methods. Subsequent consideration led, in Volume 3, to replacement of the two chapters by a single chapter, entitled Counseling. Beginning with Volumes 2 and 3, a step was taken that led later on to the separation of Comparative and Physiological Psychology. The initial plan was to have joint authorships, one author concentrating on the literature of Comparative, the other on Physiological. In this case, separation was prompted by the scarcity of specialists who felt competent to review both subjects. For Volume 5 (1954), a still different type of innovation will consist of the substitution of a chapter on Theory and Techniques of Assessment for the previous chapter on Psychodiagnostics. Other minor changes are being considered which give promise of reducing heterogeneity of topics in the several chapters. Meanwhile, since the Editorial Committee must feel its way along, it is hoped that our readers will continue to treat with tolerant attitude and understanding our liberal use of center and side headings within the chapters to delimit boundaries of subject matter.

Planning for future volumes requires special consideration of the less active areas of psychology and especially of peripheral fields that are beginning to influence psychological thought or research. These can be adequately handled by brief reviews on an annual basis or by occasional reviews at irregular intervals. Suggestions from our readers are most helpful in deciding

PREFACE

which of the areas are most deserving of review. One such suggestion bearing fruit immediately was the desirability of a chapter on Communication. This review will appear in Volume 5 (1954). Further suggestions will be welcome.

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TOPICS AND AUTHORS
ANNUAL REVIEW OF PSYCHOLOGY
VOLUME 5 (1954)

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CHILD PSYCHOLOGY^{1,2}

BY DALE B. HARRIS

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CURRENT VIEWPOINTS

Previous contributors to the *Annual Review of Psychology* have remarked that it is difficult to delineate sharply the area of child psychology; inevitably, the consideration of child development brings one to a broad, genetic viewpoint. While the four published papers of the Clark University symposium on genetic psychology illustrate clearly two different current approaches to child behavior, all agree on the importance of an inclusive, developmental frame of reference. Werner (157), Frank (36), and Anna Freud (38) use the approach of genetic psychology—that of utilizing the past to understand the present. Sears (126), as an experimentalist interested in theory, holds that it is more important “to discover principles that will predict future behavior than to discover descriptive patterns that give an illusion of understanding a child’s past.”

Werner believes that child psychology has forsaken its dynamic and organismic heritage and looks to clinicians for tools, methodology, and concepts of child care and education to replace it in its rightful domain. Anna Freud expects more study of the processes whereby character types in the psychoanalytic sense develop. Frank appeals for techniques to analyze the complex interrelations and transactions between personality and its environment, in which process personality plays a creative and purposeful role.

Sears would have child psychology make predictions in terms of variables on which the organism’s status has been determined, and, by way of illustration, refers to his studies of children’s aggressive behaviors exhibited in doll play. Correlations significantly greater than zero obtain between the mother’s punitiveness in child rearing and the child’s aggression in the doll play experiment. When the aggressive remarks of the child are systematically permitted or reproved, the amount of aggressive behavior exhibited by the child changes according to expectancy. Such findings, Sears believes, may afford a basis for clinical predictions. His correlations, of the order of $+ .20$ to $+ .50$, do not actually reduce the error of estimate by very much, even though statistically significant. Elsewhere, Sears (125) has presented a theoretical framework for the study of personality in terms of experimental work with children. His argument concerning the diadic situation presents

¹ The survey of the literature to which this review pertains was completed in May 1952. Mr. Richard Walker, teaching assistant, assisted in collating the bibliography, from which the survey was drawn.

² The following abbreviations were used in this review: WISC (Wechsler Intelligence Scale for Children); PGR (psychogalvanic reflex); TAT (Thematic Apperception Test); SES (socioeconomic status).

in systematic terms the interactive situation commonly studied by child psychologists interested in development of social behavior. This concept is at least as old as J. M. Baldwin in child psychology. Sears admits that recognizing the individual's changed potentiality for action as partly a product of his own action complicates his theory construction, which is the very point stressed so emphatically by Frank (36). Sears, however, expects ultimately to write linear equations in terms of single variables.

INFANT BEHAVIOR

Of interest to child psychologists is the rapidly growing literature in comparative psychology dealing with infantile experience and later behavior. This area has become so important that a special conference considered the effects of early experience on mental health (122), bringing together material from child development and animal studies. Published studies include the experimental work of Hall & Whiteman (46) and of Kahn (67) who subjected very young mice to traumatic stimulation. Both studies showed measurable effects on later, adult behavior. McKelvey & Marx (94) found that food deprivation in infancy had no effect on hoarding behavior in the adult rat. Fredericson (37), however, found that adult mice deprived of food in infancy competed more vigorously for food than did controls, even when not hungry. He suggests that the effect of only one training trial is measurable in adult life. Sucking deprivation in puppies was studied by Ross (120). Time sampling tests showed much greater sucking activity in the hand fed pups, although no differences between experimental and control animals appeared in specific motor items. The mother-cared animals "made a better appearance in every way," and several of the hand-fed pups died.

The effects of deprivation of mother love on human infants have been described by Robertson & Bowlby (117) and by Roudinesco *et al.* (119), in studies sponsored by the International Children's Council. Working independently in France and England, in the one case in a welfare institution for short-time care of children whose parents need to make emergency arrangements for them, and in the other in a children's hospital, the writers arrive at similar conclusions. Separation from the mother is a traumatic experience for all infants between the ages of 12 and 24 months. Even when the child has enough poise to make an "initial adjustment," there are signs of perturbation which inevitably break through within a few days. Those children who cease overt protest and apparently "adjust" after a time, also show a continuing unhappiness.

Nissen, Chow & Semmes (104) have contributed a most interesting observation on an infant chimpanzee whose opportunities for kinesthetic and manipulative experience were sharply limited. (See Chapter by Ruch, "Somesthesia and the Chemical Senses.") This study combined with the few reliable observations available on infants from extremely limited environments suggest some important conclusions for the whole problem of maturation and learning in human infants. Certainly they question the easy general-

izations sometimes made concerning maturation and learning from studies of amblystoma and chicks.

Comments on feral children as illustrating the effects of serious deprivation in early years continue to be made in elementary texts, asserts Dennis (29). Therefore, he takes occasion to reaffirm his earlier position, pointing out that six cases are on record, in which the age when the child presumably entered the limited animal environment is known. All were at least three or four years old, and if they were normal children, primary socialization with respect to foods, eating habits, and communication must have been established. Dennis' point is that instead of proving the fixity of habits established in early childhood, as these studies are frequently quoted to indicate, they prove quite the opposite!

Hayes's description of rearing an infant chimpanzee (54) illustrates environmental enrichment rather than deprivation. The study of language is of particular theoretical interest, suggesting that while the infant chimpanzee goes through a developmental stage of babbling similar to that of the human infant, neurological and muscular mechanisms are such that he does not retain these sounds and build them into a speech form. Nor is this merely a matter of intellectual retardation. Up to 3 years, the animal handled performance tests in a manner comparable to the average human child of similar chronological age.

The hypothesis of developmental pre-eminence—that there are critical periods in growth, times when subjects are relatively more susceptible to particular learning influences—has been applied by Scott & Marston (123) to social behavior of puppies. They describe four distinct levels in the emergence of social behavior prior to sexual maturity. Using timed observations and experimental tests, they have shown that disturbances in social learnings are most likely to occur during the transition periods between levels or phases, at the time of forming new social relationships. Stendler (131) develops this hypothesis in relation to child socialization, using the language and viewpoint of Sears concerning habits of dependency, and of psychoanalysis concerning ego organization and character formation. The human infant quickly learns dependency on the mother because she has secondary reward value, being associated with hunger and pain reduction. But she also orients the young child toward independence. The end of the first year, when the child first tries to control its mother, becomes a critical period in the development of independence. Another critical period occurs when the social demands on the child change as it develops free locomotion. Stendler believes that traumatic episodes occurring in these critical periods may produce overdependency.

Three papers bear upon the problem of maturation and learning. Hofstaetter (57) expresses mathematically the relative rates at which development occurs in the chimpanzee and man. The correlation between the time of appearance for 13 behaviors is $+ .96$, with a marked linear trend. Walking alone comes considerably earlier in man relative to other behavioral develop-

ments. By relating the above data to data on the growth of brain weight in respect to total body mass, Hofstaetter arrives at the hypothesis that "the speed of maturation is inversely related to the cephalization coefficient of the species." Bousfield & Orbison (14) discuss in some detail the development of emotional behavior. Remarking that the early emotional responses of infants are characteristically brief, intense, frequent and undifferentiated, they show how these characteristics follow from two physiological assumptions: (a) "the infant is essentially a pre-corticate organism and the establishment of cortical control, especially in the frontal lobes, is not achieved until adulthood," and (b) "the infant is relatively lacking in endocrine products which sustain some of the physiological responses to stress." Their discussion relates their observations to Hebb's thinking on the organization of behavior (55) and to Banham-Bridges' work on the organization of emotional responses in infants and young children (8). Child psychologists will be interested in the progressive modification of emotion in later maturity described by Banham (7) in relation to her earlier work with the young child (8). The differentiation from a primitive infantile "emotion" of excitement, through "distress" and "delight" into a complicated pattern of reaction forms is followed at an older age by "a certain amount of consolidation, constriction and disintegration."

PERCEPTION

Malrieu (85) describes in great detail the processes by which his own infant made its first perceptual, auditory, and social adjustments, with particular emphasis on responses to visual stimuli. Piaget & Lambercier (111) offer an apparatus for studying perception in depth in the child. Their distinctions between "objective size," "projective size," and "apparent size" are somewhat arbitrary, and it is difficult to see how children could comprehend them and follow the authors' complicated instructions. For instance, the task requires the estimation of size of a distant object projected on a plane much nearer the eye. Notwithstanding these difficulties, the authors report that seven- and eight-year-old children are superior to adults in estimations of projective size. The ability falls off in successive age groups to the poorest performance at years 10 to 12 and then rises to the adult level. Because children younger than seven could not understand the instructions, data are lacking, but the authors infer a still higher ability to estimate projective size. One wonders what connection may exist between the child's progress in representative drawing and this decline in capacity from 6 to 12 years. Piaget offers an extensive discussion of his results which breaks rather sharply with both the classical and Gestalt theories of perception.

PHYSICAL GROWTH

Meredith and others (96, 97, 98) have been successful in devising reliable indices, based on simple linear measurements of arm and leg length and circumference, which describe a slender-to-stocky continuum in four- to

eight-year-old boys and girls. While Meredith has not yet compared them with ratings of body build as measures of body type, the indices do intercorrelate highly and reveal the "slimming out" with age which occurs in middle childhood. The results of Meredith's comparison with ratings of body build will be awaited with interest. Massler & Suher (91) have also been successful in using linear measurements to delineate seven groups, ranged on a body-build continuum from ectomorphy to endomorphy. Results correlate positively with "subjective somatotyping" and with typing according to Wetzel's grid method. Body weights predicted by a formula which includes standing height and calf girth correlated $+0.99$ with actual body weights. Unfortunately, the correlations appear to have been based on the same data which yielded the regression weights for the formula. These researchers, like Meredith, find different patterns for girls and boys.

Two papers relate physique and puberal status to leadership and personality characteristics in junior high school boys. Latham (75) discovered that the puberal status (Crampton's criterion of pubic hair development) of athletic leaders was more advanced than that of nonleaders, but no such difference appeared in social leadership (number of elected positions held). The highest correlation (tetrachoric) was $+0.30$. Hanley (47) determined the relationship between a somatotype and "guess-who" measures of children's reputation. Since sociometric records were available on a longitudinal basis, it was possible to study records obtained prior to physical maturity in relation to the later status achieved by the boys. Ten of the 14 traits expected to correlate with mesomorphy were significant at the 5 per cent level. Seven of the 17 traits expected to correlate with ectomorphy were significant at the 5 per cent level. These relationships are scarcely great enough to indicate a socially significant connection between body type and personality.

Two interesting studies find evidence of environmental impact on growth among rapidly disappearing racial stocks. Ohwaki (106) compared a group of Aino children (of Caucasian background) in northern Japan, with Japanese and mixed-bloods living in the same villages. Although differences are not large, they are quite consistent. Japanese boys and girls tend to be taller up to age 12 and shorter thereafter. The Aino children appear to be slightly stronger. Ohwaki remarks that the racial advantage of the Aino in size and strength has been reduced considerably through changes in culture and dietary habits, and increase of disease. Garn & Moorrees (40) studied Aleut children who were violently disturbed by the war. Some had been evacuated to Japan, and others to Alaska; all were repatriated following the war. Their traditional diet was considerably changed by both evacuating nations. The trend lines of the growth data are above those for Eskimo children, who are genetically and culturally close to the Aleuts. Chi square tests indicate a significant statistical difference. Younger Aleut children, those under 10, are relatively taller, but the 10- to 18-year-olds, conform more to the standards for Eskimo children. In body type (according to Wetzel's grid) these Aleut children compare with Aleuts from St. Paul

Island, who have had much better nutrition and medical care over a long period of time.

INTELLECTUAL ORGANIZATION AND DEVELOPMENT

While no new major tests appeared for the assessment of child intelligence, the evaluation of existing instruments continued. Kent (70) has published descriptions, directions, and tentative norms for three performance tests which she finds of considerable relevance to young children. The use of the Porteus Mazes has been reviewed and evaluated by Tizard (143) who finds much to commend, though he criticizes instructions for administration. Coppinger & Ammons (25) report vocabulary norms for Negro children, based on five boys and five girls at each grade level from one to eight, with socioeconomic distribution paralleling the distribution for southern Negro males. These results indicate that the Negro children perform in a manner roughly equivalent to white children two years their junior. Correlations of .84 and .81 between the Ammons picture test and the Binet vocabulary were obtained in this study. This interesting test deserves much more extensive standardization.

During the year a number of studies evaluated and compared the Wechsler Intelligence Scale for Children with the Stanford-Binet. Pastovic & Guthrie (109) mention four unpublished studies and present data for a fifth. These and other studies are summarized in the table on following page. In general these studies indicate that the WISC² is not equivalent to the Stanford Binet and that higher mean performance IQs than verbal IQs are found over a wide range of intelligence. Wechsler (154) renews his argument for an IQ based on the standard deviation of performances at each age in place of the traditional mental age and suggests several ways of calculating an equivalent mental age from his IQ.

Cattell (20) reports norms for a culture free intelligence scale. This test gives a good progression with age, though the curve flattens out earlier than has been found with verbal or other culturally involved tests. Of interest is Cattell's claim that as the scholastic contamination in intelligence tests is reduced, the standard deviation of IQs increases from about 12 (1916 revision of Stanford Binet) to 24 or 25 points (his own scale).

McKee (93) discusses the Thurstone Tests of Primary Mental Abilities applied to superior kindergarten and first grade children. Although the tests were interesting to the children, the administration time was too long, and there was not sufficient ceiling for bright seven-year-olds and very superior six-year-olds. Doppelt & Bennett (30) report a longitudinal study of the Differential Aptitude Tests for some 300 children tested in the ninth and twelfth grades. Correlations indicate high stability of relative position in the group, despite growth in the capacities measured. The verbal reasoning and language usage subtests showed the highest correlations over the four year period. Clerical speed and accuracy measures showed the lowest correlations. Girls were more stable than boys in abstract reasoning and space relations.

TABLE I
COMPARATIVE STUDIES OF THE WECHSLER WITH OTHER
INTELLIGENCE TESTS FOR CHILDREN

Author	Subjects	WISC		Other Tests		Correlation	
		\bar{X}	S.D.	\bar{X}	S.D.		
Pastovic & Guthrie (109)	50 Kindergarten	(V)	101.6		(Binet)	.63	
		(P)	104.2		113.0	.57	
		(FS)	103.2			.71	
Pastovic	50 Second Grade	(V)	108.6		(Binet)	.82	
		(P)	112.7		115.1	.71	
		(FS)	111.5			.88	
Clarke	85 Fifth Grade	(V)	96.1		(Binet)	.83	
		(P)	99.8		96.1	.57	
		(FS)	97.6			.79	
McBrearty	52 Fifth Grade	(V)	95.8		(Arthur)	.55	
		(P)	99.0		101.8	.65	
		(FS)	97.1			.71	
Nale	104 Defectives	(FS)	57.8		(Binet)	.91	
					55.4		
Rapaport	100 Second grade	(V)	89.9		(Binet)	.79	
		(P)	91.6		97.0	.74	
		(FS)	89.6			.85	
Frandsen & Higginson (35)	54 Fourth Grade	(V)	100.9	12.2	(Binet)	.71	
		(P)	103.5	11.2	105.8	11.15	.63
		(FS)	102.4	11.2			.80
	(other findings: correlate order of $+.63$ with Stanford Achievement Test)						
Krugman <i>et al.</i> , (73)	322 various ages	(V)		Binet gives significantly higher mean IQs at all ages except 14-15 on FS, on P, and less often on V.		.64 to .88	
		(P)				.53 to .79	
		(FS)				.73 to .92	
(other findings: $\frac{2}{3}$ of children get S.B. IQs within 10 points of WISC FS or V; $\frac{1}{3}$ on P scale)							
Weider, Noller & Schramm (155)	106 children 5 to 7	(V)	91.9	17.1	(Binet)	.89	
		(P)	89.8	18.1	93.1	19.6	.77
		(FS)	90.0	18.9			.89

The authors wonder whether this indicates a greater uniformity of experience for girls in these areas.

Michael *et al.* (100) have examined the nature of spatial relations and visualization abilities, using 15 tests, 7 of which were devised or adapted to investigate the hypotheses that spatial relations ability is an ability to comprehend the arrangement of elements in a visual stimulus pattern with positional reference to the human body (i.e., high, low, left, right), and that visualization is an ability requiring mental manipulation of visual images and is distinct from visual memory and ability to reproduce visual images. The hypotheses were confirmed. Boys exceeded girls on tests loaded with the spatial relations factor and tended to exceed girls on tests loaded with the visualization factor. The same general pattern of six factors appeared in each test group.

Numerical abilities have been studied in three papers. Martin (88) offered patterned materials (pictures and designs) susceptible of quantification to preschool children, as well as the more usual tests of number ability. The child's verbalization concerning the materials was encouraged, recorded, and evaluated for evidence of quantitative expression. Samples of 30 children selected to represent an urban population at each age from three to seven were used. Results confirm findings of other studies and supply information concerning the child's spontaneous use of numbers and the extent of his spontaneous quantitative vocabulary. In general, boys use more spontaneous expressions of quantity and girls excel on formal tests of number ability, though in the upper socioeconomic levels, the reverse is true. The correlation between the tests of number ability and the child's spontaneous use of quantitative expression is positive but slight.

Ilg & Ames (60) continue their developmental studies of intellectual performances in a report on arithmetic. Using a sample of 50 children at each six-month interval in the preschool years (not all of the subjects being examined consistently from year to year), they describe an arithmetic gradient. Emphasizing the point that mistakes are not "all or none," but that there are degrees of error, they show that the type of error made depends upon the process the child uses to arrive at an answer. Some errors are "self correcting," in that, although wrong in conclusion, the child is correctly oriented to the problem. This type of error occurs frequently and may actually be taken to indicate the child's developmental stage in arithmetic skill. Ilg & Ames concur with Martin (88) when they suggest that what the child says while engaged in arithmetical operations may be very important in indicating the nature of his development to date. They and Martin agree in noting that boys are more aware and interested in arithmetic manipulations. While Martin found girls superior on tests of number ability, Ilg & Ames report boys superior in the manipulation of numbers. Of educational importance is the observation that young children six to seven years of age enjoy oral arithmetic very much, though they struggle with written figures. The authors criticize the arithmetic curriculum and suggest that the arithmetic

operations be redeployed in terms of the developmental stages reached by children. If this reassignment of learning operations is to be accomplished we must have more adequate studies of the arithmetic gradient. The mixing of cross-sectional and longitudinal samples is poor procedure for establishing status norms.

In a carefully controlled study, Brownell (16) reports on arithmetical readiness in the sixth grade. If 80 per cent accuracy on the test is taken as "readiness," nearly half of the children in the sixth grade were not ready for two-place divisors. Of interest to child psychologists who are impressed with the negative motivational effects of some types of instruction is Brownell's footnote reference to an unpublished study which shows that children, when not penalized for purely computational errors, learn to divide by two-place numbers without much trouble!

Other papers on concept formation support the view of development of perception, abstraction, and generalization that is now generally held by child psychologists. Springer (130) has studied the development of time concepts by asking children four to six years of age to draw a clock. She finds eight stages in the development of the concept. Only 13.6 per cent of the four-year-olds failed to show any knowledge of the clock, and one five-year-old and nine six-year-olds reached stage eight—(showing the shape of the clock as varied, hands correctly placed, 12 numbers shown clockwise, carefully and correctly spaced). Michaud (101) used a simple drawing test to locate two stages of maturity in the development of conceptual thinking. Practical thinking develops rapidly to ages 12 or 13, and conceptual thinking emerges at about 10 years and is built on the basis afforded by practical thinking. Malrieu (84) discusses children's drawings as an avenue to the child's understanding of the world, stressing the study of the sentiments of the child who draws as a fundamental contribution to art education of children.

Feifel (33) has analyzed the types of erroneous definitions given to Stanford Binet vocabulary items by children 6 to 14 years of age. Six types of wrong definitions are cited. The use of "clang" associations declines with age. There are no real age changes on misinterpretation, incorrect demonstration, and repetition. Outright errors in definition decrease significantly at each age except at 13 and 14. The author concludes that there are "no sharply demarcated patterns characterizing stages in the child's development of thinking."

Cowen & Thompson (26) have attacked the problem of mental rigidity in children, using the "water jar" type of problem. Seventeen eighth grade children who used simple solutions to at least three of four test problems, following work with "Einstellung" problems, were compared with 17 who used complex solutions to at least three of the four test problems. There were no differences between these contrasted groups in age, school achievement, intelligence, or sex composition. The Bell and California Personality tests revealed no significant differences between the groups, but certain Rorschach

differences appeared. Judges gave "poorer" ratings to Rorschach protocols of the rigid group (which persisted in the Einstellung set). Two Rorschach scores differentiated the groups at the 1 per cent level of significance and five at the 5 per cent level. These scores were interpreted to mean that the rigid group showed limited productivity and imaginativeness, inability to perceive complex relationships and to integrate constructively, generalized suppression of emotional expression with respect both to rich inner creativity and to interaction with outer reality, and a restricted range of interests. The authors say:

other things being equal, knowledge of a person's position on a scale of "Einstellung" rigidity permits deductions within broad limits about his behavior in certain other situations. This type of deduction constitutes prediction at a primitive level, a goal of all beginning scientific endeavor.

Even though some differences significant at the 5 per cent level are noted, this reviewer wishes to underscore the word "primitive" in the above quotation. There is certainly little warrant for discussing individual prediction in a sophisticated sense with the few and very gross differences located by the Rorschach in this study.

A number of papers report the effect of environmental conditions on intelligence. McCandless (92) reviews the literature on environment and intelligence, covering social class research, research with infants, foster child studies, nursery school studies, and "sub-cultural" mental deficiency studies. Usdin & Weil (149) followed a group of 41 children who, according to hospital records, had had difficulty establishing breathing at birth (apneic for 3 to 10 min.) and compared them at age 14 with a control group. No differences in range or mean of IQ appeared. De Groot (28) reports additional data on Netherland children. The average IQ of school applicants, which was 100 to 103 in the five-year period following 1938, dropped to 95 or 96 in the period 1944 to 1948 and rose to about 100 in 1949 to 1950. De Groot argues that the IQ can only be interpreted in relation to a person's developmental history and sees the above decline as the effect of war on successive classes of children.

Richards (116) argues that IQ changes should be thought of as more than "mere unreliability" of measurement. Weisskopf (156) develops a similar thesis. Reasoning from pathological cases she says:

the same emotional factors which cause pathological intellectual blocking in some individuals may, by being present in different degrees, become responsible for quantitative and qualitative differences in intelligence within the normal range.

Gilliland (41) has used his intelligence scale for infants to evaluate race and socioeconomic status differences. Various comparisons reveal no particular significant differences between broadly different socioeconomic levels. Three separate studies involving Negro and white infants are reported; one shows Negro children significantly higher than white, and the other two report Negroes slightly but insignificantly higher. Infants reared in an in-

stitution score significantly below infants reared at home, though both groups have parents of comparable socioeconomic background. Cook & Arthur (24) state that Mexican children in Minnesota do much better on the Arthur I than on the Stanford Binet (L). They comment that the Mexican children show the trial and error approach to difficult items characteristic of American white children, although American Indian children show a consistent tendency to plan their work in advance, thereby penalizing themselves on timed items.

COMMUNICATION PERFORMANCES

Under this topic are grouped studies of speech, hearing, and writing processes. Leischner (77) offers an interesting notion concerning the early development of speech. He holds that musical and articulatory elements constitute the two principal aspects of sound. The musical one develops early and is prominent in the infant's first speech sounds. Articulation develops more slowly and as a background feature. This process is reversed in the decay of speech performance, several cases of which Leischner describes in great detail. Short meaningless words give way to articles, prepositions, and interjections, which in turn give way to meaningless syllables and eventually to rhythmic sounds.

Irwin (61), continuing his series of publications on infant speech, offers results of a longitudinal study of initial consonants from birth to two and one-half years. He finds that initial consonants develop according to a linear pattern, medial consonants at a decelerating rate, and final consonants at an accelerating rate. Initial consonants occur most frequently in infants, medials next, and finals least often.

Quite devastating to studies such as Irwin's is Lynip's study (81) of infant sounds. Utilizing the sound spectrograph developed by the Bell Telephone Laboratories, an electronic device which provides a graphic record, Lynip recorded the early sounds produced by one infant, beginning with the birth cry. His results show why phoneticians so completely disagree on the qualitative analysis of the earliest infant sounds. Translated into a visible pattern by the sound spectrograph, these very early infant sounds consisted of "blur of indistinguishable vibrations . . . utterly different from any sound characteristic of adult vowel sounds." Although these sounds soon came superficially to resemble adult vowel sounds to the listener, the spectrograph showed them actually to be quite different. By six weeks, the sounds had developed to what adults usually call crying and whimpering, but a continuing change of pattern was recorded by the spectrograph. By 38 weeks, the infant was producing imitative sounds. This study does not support either of the two classical views of imitation—that imitation is the exact reproduction of adult sounds, or that the infant reproduces only those sounds actually in his repertoire. When the child "imitates," the visible record of his sound patterns is distinctly different from that of the adult sounds they superficially resemble. This study shows a constantly changing

production of sounds, with speech emerging as a gradual reduction of variability in specific patterns. The need to study inter-child variability by this method is at once apparent.

Focussing upon the content rather than form of speech, Albright & Albright (3) elicited verbal associations to pictures of 10 common objects, such as bed, train, table, house, and spoon. Children 28 to 76 months old somewhat above average in intelligence served as subjects. An average response to a picture contained about five words. While there were no significant age changes in the mean number of responses, there was some increase in variability. The nursery school children gave more naming responses and tended to use more nouns, while kindergarten children used more verbal nouns. Kindergarten children also used more individual response words and more unique response words. Although effects of personal experience were expected, none were found. These and "localisms" appear to be a later acquisition.

Beebe's review (9) of methods of testing the hearing of young children is descriptive rather than evaluative and gives footnote reference only to the important work of Hardy and his colleagues (48,49). The latter used a conditioned PGR² to a mild shock associated with a pure tone as a warning signal to test hearing in very young children. For school children this method gave results which lie within a five decibel range compared with the ordinary method. Johnston (64) and Nielsen (103) offer group screening tests of hearing, utilizing a multiple headset, which can be applied to children as young as six years. Nielsen's method utilizes a paper-pencil mode of response and is probably superior to Johnston's, for group testing which requires children to give a hand signal when they hear the tone.

Ames & Ilg (5) continue their report on trends in writing behavior. Directionality in copying and drawing were described in earlier papers; this article reports the child's handling of letters and figures, special attention being given to errors made in producing and in reproducing figures and letters. The authors assert that individual differences in "readiness for writing" exceed the developmental differences now recognized in schools, that most writing standards are far too high for the motor capacities of primary children, and that many so-called "errors" are normal deviations or developmental trends, being shown by a majority of children. The authors also insist that many of these errors are self-corrective in the developmental process. The beginning of individual handwriting style is observed to occur at about nine.

CHILD SOCIALIZATION AND THE PARENT-CHILD RELATIONSHIP

Hieronymus (56) examines social class attitudes toward education. Testing 610 ninth grade children in the Middle West, he used a modified Sims score card, an attitude toward education scale, a scale of socioeconomic expectation (social values aspired to), and measures of ability and achievement. Socioeconomic score is more closely related to the child's level of socioeconomic expectation than is intelligence. With intelligence partialled out,

the correlations between socioeconomic expectation and socioeconomic status approximate $+ .52$ for both boys and girls. Socioeconomic status correlates $+ .30$ with attitude toward education, a value about the same as that obtained between test intelligence and attitude toward education. Socioeconomic expectation and attitude toward education correlate $+ .50$. From these data the author infers that social class factors have more to do with social goals and goal striving than does intelligence. Centers (21) has demonstrated that adolescents show the same class-related social and political attitudes, and the same social class identifications as adults in their respective social strata.

Abrahamson (1), assuming that innate intelligence is distributed among school children independently of socioeconomic status, investigated the proportion of marks given to children of different social class groups in a number of schools. Chi-square tests show that teachers give a disproportionately large number of high marks to middle-class children. The Ohio Social Acceptance Scale indicates that middle-class children receive relatively more acceptance choices from their classmates. The same finding was confirmed with respect to offices held in school organizations. Abrahamson concludes that teachers and children tend to favor middle as opposed to lower-class children. A somewhat different interpretation would be made by one who does not assume that abilities are uniformly distributed in the population.

Based on observation of youth groups, Maas (82) questions the usual formulation of outcomes of class differences in child training. He agrees that the lower-class child experiences greater social freedom and expresses more overt physical aggressions than the middle-class child, but doubts that the lower-class child has much psychological freedom. The closed, hierarchical, rigid parent-child relationships in the lower classes cause children to fear parents more than in the middle classes, even though lower-class children may have greater physical and social freedom. The lower-class child may have more feelings of rejection and unworthiness as a result of disobedience. On the other hand, middle-class children have more equal access to both father and mother, and consequently neither fear nor identify with the potentially threatening power of adults so much. Maas notes fewer dependent relationships and less bullying, submissive, and hierarchical relationships in middle-class peer groups. The range of peer relationships is wider and more varied for middle-class children. Thus the greater psychological openness of middle-class families may actually be more growth-promoting than the apparently greater freedom of the lower-class families.

Two studies discuss trends in the popular literature of child care. Vincent (150) analyzed 644 popular articles published between 1890 and 1948. Although some changes in emphasis appear, there is little evidence that this literature has much impact on the folkways, much less on character structure of children! Vincent believes the literature actually has followed rather than paced the changing middle-class ideology. Scientific evidence apropos the various arguments concerning breast versus bottle feeding, formulas, and the like, is quite absent. He notes in modern writing a trend away from dogma-

tism and toward offering alternatives and suggestions, but no increase in articles using empirical evidence as a basis for argument. Klatskin (72) reports a study from the Yale rooming-in project indicating that middle-class propaganda may influence child care practices. When divided into broad socioeconomic classes, the respondents showed no differences in self demand feeding, bottle feeding, or the duration of breast feeding practices. There were no class differences in father participation or in definite bedtime. The author attributes the fact that his results differ from those of other studies to the effect of "rooming-in" ideology, especially in regard to emphasis on feeding and sleeping practices and to father participation. Upper-class mothers were less strict on toilet training and trained more frequently after the first year. The usual class difference appears, presumably because nothing was said about toilet training in the indoctrination.

Social class differences affect the parent-child relationship in adolescence, states Nye (105), who found that higher socioeconomic groups were frequently favored by items on a check list of parent behavior, completed by 1500 high school youth. The socioeconomic differential persisted when the effects of rural residence, broken home, family size, and working mother were held constant.

Kanner (68) remarks that the discovery of the significance of maternal attitudes for child behavior has produced a literature overstressing the point, and tending to make mothers insecure and overconcerned about their handling of children, with consequent bad effects on children. Bettelheim (10) finds middle-class mores enforcing too many double standards and over-emphasizing intellectualism, which tends to destroy the individual's libidinous energy for living.

Counter to the argument that emphasis on the training of adults for parenthood may produce anxieties, Stott & Berson (136) found positive results with an attitude scale given before and after eight weeks of instruction to 28 couples. The changes in individual attitudes were, with few exceptions, toward a permissive and away from an authoritarian view of child care and discipline. A control group which received only the usual obstetrical advice showed no such significant change. Changes in attitudes were greatest with respect to discipline, with changes in feeding, toileting, and sleep following in that order.

Remmers & Drucker (114) report an interesting study of the attitudes of high school youth toward children and child behavior which has both social and methodological significance. They adopted an analysis of variance design to include four dichotomous factors (sex, mother's education, father's education, home environment level) and one four-fold factor (high school class). Twenty-five cases were selected from the supply for each of 64 cells, representing all combinations of the factors. All interactions were found to be insignificant except the first order interaction of sex and grade. The higher the grade, the higher the score, other things being equal, and girls scored higher than boys. The relationship of score to parents' education was also significant, but that to home environment level was not. The authors point

out that a sex difference in attitudes towards children and child behavior exists well before the end of high school. It is Remmers' belief that such sex differences in attitude may increase after marriage. This hypothesis is susceptible to study, and it would be of interest to see whether a preparental course such as that described by Stott & Berson (136) would serve to decrease such a difference.

In large families, Bossard & Sanger (13) find that social roles are more clearly defined and reinforced and that individuals tend to develop patterns of philosophic acceptance of events. Specialization develops in the large family, with attendant increase in interdependence. Presumably such special patterns have significance for personality formation. Bossard (12) also presents some interesting observations on the significance of family visiting, which provides the child with a much broader base of experience. Approximately one-sixth of the adolescents he interviewed did not go visiting as children; the controlling reason was chiefly the attitude of the parent, who in most of these cases was domineering or distrustful of people, or considered other people to be a bad influence. Contributions to child development from visiting include intellectual gains from travel, enhancement of concepts of space and time, new ideas of freedom and family practices, ego reinforcement from attention and praise given to children (which as often as not was balanced by experiences of humiliation and shame), new insights into life and values (mentioned by more than half the children), contributions to sex education (mentioned by one-fourth of children), and a greater appreciation of their own homes and parents as the result of visiting.

Elias (31) has contributed a questionnaire measure of "homelessness" (presumably a measure of emotional distance or identification), validated by the contrasted group method, utilizing juvenile delinquents. When age, sex, and extreme poverty are controlled, no racial, cultural, religious, or rural-urban differences appear. Scores increase after age 10 and then level off in later adolescence. Presumably this increase in scores shows an increasing "homelessness" and reflects the adolescent's struggle for independence. Cass (19) used an ingenious device to study parent-child relationships and delinquency. Twenty-one adolescent delinquents were matched with well-adjusted adolescents in terms of chronological age, sex, race, and father's occupation. The children and their mothers filled out check lists, the mother taking the check list once for herself and once as her child would answer it. The children reported on parent techniques of control and responded to a series of incomplete sentences to indicate conflicts. Three measures were derived: Projection to the child, the number of items mother checked similarly on her self questionnaire and for her child, which were not checked by the child on its self questionnaire; identification, the number of items checked similarly by the mother on her self questionnaire and by the child on its self questionnaire; and awareness, the number checked by the mother for the child which were actually checked by the child also. Results significant at the 1 per cent level were as follows: delinquents report more parental control, and mothers of the delinquent have less awareness of the child's actual

personality status; delinquents report higher conflict scores. In the non-delinquent group the correlation between identification and projection to the child is $+ .44$; in the delinquent group this correlation is $+ .06$. Awareness and identification correlate only $+ .11$ in the delinquent group and only $+ .32$ for the well-adjusted group. The study adds to the accumulating evidence that delinquents may not necessarily experience more neglect or indifference from parents and may actually be subject to greater degrees of control, though perhaps experiencing less adequate understanding.

Bishop (11) observed children in play situations with their mothers and in play sessions with a "neutral adult." Specific measures of maternal control correlated from $+ .45$ to $+ .71$ with measures of the child's non-co-operation or reluctant co-operation with her. Values from $+ .42$ to $+ .62$ resulted between mother's directing, interfering, or criticism, and child's non-co-operation. Strong, emotionally toned stimulation by the mother correlated from $+ .52$ to $+ .71$ with negativism in the child. (Most values reported are significant at the five per cent level, though not at the one per cent level.) In the two sessions with the neutral adult, the child's behavior tended to resemble that expressed while with his mother, more particularly in the second session than in the first, presumably because of increasing familiarity with the strange adult.

Pauline Sears (124) reports appreciable sex differences in kind and amount of doll-play aggression which increase between three and five years. Boys are more violent, rough, and physically hurtful and turn more hostility toward the father doll. Older sibs are frequently less aggressive than younger siblings or only children. Girls' doll play is little affected by the father's absence from the home; boys whose fathers are absent show less aggression. The author concludes that "children's behavior in doll play reflects the broader habits of aggression which they employ in everyday life." The phantasy components of doll play, at least as represented under conditions of this experiment, do not reverse the behavior which occurs in more realistic situations. Hollenberg & Sperry (58) also describe a number of antecedents of aggression and effects of frustration in doll play of preschool children.

The significance of parents in development of the ethnic attitude environment of children is considered by Radke-Yarrow, Trager & Miller (112). For example, about a third of Protestant and Catholic parents used hostile descriptions and stereotypes in explaining race and religious differences to their children. About one-fourth saw similarities in all groups, and a majority included statements which were neutral or friendly in tone. Three-fourths of the whites and half of the Negro families interviewed believed their own children should be taught to recognize differences between groups, but only about 4 per cent obviously sought to instill prejudice. There is considerable evidence for a concomitance of attitudes in parents and children. The authors conclude that

parents' explanations of group differences indicate either a marked lack of information about and understanding of cultural differences, or at least an inability to express these differences. In addition, many of the parents' perceptions of groups appear to be

inextricably bound up with their feelings about these groups and influenced by superstitions, misconceptions and feelings of unfriendliness.

Stendler & Young (133) report a follow-up of an earlier study (134) showing that the general improvements in behavior reported by mothers as resulting from the child's entering school continue through the first year, except for "patience" and "obedience." Upper middle-class mothers are most likely to criticize the school to the investigators, but also most likely to defend the school to the child when the child complains.

Socialization studies are also found in the Japanese literature. Ohwaki (107) studied Aino and Japanese children living in the same villages. Although these two racially different groups do not now differ in language, manners, economy, or political rights, certain cultural differences do persist. Ohwaki notes that Japanese parents had more education, occupied a better economic status, and read more books and magazines. Aino and Japanese families were of about the same size and exhibited religious activities in about the same proportion. In handling children, the Japanese father was more strict, the Japanese mother less indulgent, than their Aino counterparts. The Aino parents appear to reproach school achievement less often and to chat less with their children. Aino children help more often with family chores and are more likely to miss school for this reason. Aino parents praise their children about as much as Japanese but give less pocket money (reflecting the economic difference). The great majority of both Aino and Japanese children like school and their teachers. In respect to school subjects, the Aino children prefer physical activities somewhat more than do Japanese; for other subjects, the Japanese and Aino preferences are quite similar. In a sociometric choice test, children tend to name their own racial group preferentially. Somewhat larger proportions of Aino children describe having been despised or teased, or having experienced shame or inferiority feelings.

DEVELOPMENT OF SOCIAL BEHAVIOR

Only three papers have been examined which may be classified as descriptive studies of the development of social habits or skills in children; all others in this category are oriented from the viewpoint of group dynamics and climate studies. Phillips and his associates (110) describe the assimilation of a new child into the play group. He observed four play groups, each composed of three children, six to seven years of age. Into each group a new child, similar to the other children in sex and age, was introduced. The new child characteristically made the first overtures; he tended to imitate the remarks and activities of the most active child in the nucleus group and his movement into the nucleus group fell into five distinct levels of integration.

Spontaneous groups of children and adolescents in Israel interested Wolman (158), who gathered data from some 2500 boys and girls from 8 to 20 years of age. The 8- to 12-year-old group gave the highest percentage of gangs and the highest percentage of children from the total group belonging to them. Gangs at this age emphasize romantic names, have a unisexual

character, and observe strict discipline and hierarchic, authoritarian social organization. Gangs in the 12- to 14-year group differ radically from those of previous age groups. They are smaller in numbers; few have characteristic names; they endure over a shorter period of time. After 14 years of age, only maladjusted individuals with rather low school records and poor intelligence tend to belong to gangs. Many of these children show delinquent tendencies. Wolman concludes that the gang and its loyalties are too narrow and too confined to suit the enlarged interest and loyalties of midadolescence.

Mason's study (90) of elected leaders in an elementary group agrees with much extant research in finding that leaders made superior scores on a group personality test and are described as dependable, resourceful, self confident, quiet, and capable.

While many studies have appeared this year describing the effect of group contexts or climates on the behavior of group members, relatively few of these have been actual research projects. Mensh & Mason (95) compared girls in grades five through eight attending traditional and progressive schools. The Rogers Test of Personality showed no particular differences between pupils in the different schools, but the Rosenzweig Picture Frustration test indicated that students attending the traditional (authoritarian) school tend toward greater conformity. However, the conforming response mean score for the traditional group is well within the normal range as described by Rosenzweig. To dichotomize schools as traditional or progressive is a gross oversimplification. Mensh & Mason could profitably have spent more time establishing their criteria in terms of actual teacher behaviors. Wright, Barker, *et al.* (159) continue their researches into psychological ecology. Classroom behavior, as contrasted with activity in behavior settings relatively free from adult control, may be described as follows: low in intensity, frequently at a low or medium efficiency rate, minimal in creativity, more likely to be co-operative. The child's cognitive field in the classroom is small and relatively unclear, and the school makes much use of social pressure methods of motivation resulting in many social interactions.

Stendler *et al.* (132) adapted the "social climate" method to a study of motivation. Groups of children working at a group task showed more positive than negative social interactions when promised a group reward. When a reward was given only to the best worker in the group, negative and positive behaviors were equal in frequency. In free play following the work sessions, there appeared to be no carry over of behaviors exhibited during work.

Two studies, extremely ingenious in design, by Lippitt and colleagues carry forward the Lewinian concept of group atmospheres. Both deal with "behavior contagion," a term used to designate a social effect which in an earlier day might have been referred to the individual and called "suggestibility." Both studies used 10- to 14-year-old boys. The laboratory experiments (45) used paid subjects and a "collaborator"—a boy of the same age-stranger to the subjects, who "set the stage" in various ways. The experiments suggest that the subject's degree of competitiveness correlates positively with his tendency to be influenced by (to "contage from") interrupting

activities staged by the collaborator. Other experiments, in which the collaborator introduced experimental influences during rest periods in a work session, showed a number of clear effects of the "atmospheres" created by a peer who is a stranger. In a second article, Lippitt *et al.* (78) studied contagion effects in summer camps where the basic social unit was a cabin of eight boys. In each group, boys rated themselves and others on a number of variables, one being power status. Boys in groups clearly recognize that there are large differences in the ability to get others to do what one wants, and they agree on the boys who are deviates in this ability. Boys tend to imitate the behavior ("to contag from the behavior") of those to whom they attribute power to influence the group. The average group member is more likely to accept direct attempts to influence him (as distinguished from contagion influence) when they come from high power figures. Self ratings of power tend to be consistent with ratings attributed by others. The "recipient of attributed power" (a boy high in the ability to influence others) will make more attempts, and more directive attempts, to influence others and be relatively more successful in his attempts. Significantly enough, high power subjects do not differ particularly from average or low power subjects in height or weight or in amount of camping experience. They are better liked, and also are higher in personal prowess (fighting ability). No relationship appeared between attributed power and the boy's impulse-control as rated by a camp counselor.

These studies attack significant problems and some questions remain for future study. Would different combinations of personalities in the experimental situations change the strength of relationships found? What would happen, for example, if the "personality" of the collaborator became a variable to be systematically changed through the standard situations? What happens in a cabin group where two very forceful personalities are present? What happens where no particularly strong personality is present? There is much evidence to assert that the personality of the leader is not altogether a product of the particular forces at work in the group but would make itself felt in a variety of groups which he might enter.

SOCIOMETRIC STUDIES OF SOCIAL BEHAVIOR

Byrd (17) found only moderate correspondence between paper-pencil choices and actual choices made for the same situations later on as a part of class work. A slightly higher percentage of agreement was found when the written test was readministered. Justman & Wrightstone (66) evaluated three measures of child status and concluded that rating associates on a five-point friendship scale and naming persons liked best and least are practically interchangeable as status measures. A near-sociometric measure, naming children for positively and negatively described roles in a hypothetical play, correlated only moderately with the other two methods. Thompson & Powell (141) find that Moreno's partial rank order method gives a more stable classification of "stars" and neglectees, while a rating method (utilizing "social distance" statements) has more "face validity" and distinguishes

more accurately among isolates and rejectees. The median intercorrelation between the methods is only $+.60$. Although the authors favor their rating method on theoretical grounds, it is extremely cumbersome. Their critical discussion of the Moreno technique is particularly noteworthy.

Sociometric and near-sociometric devices continue to give information concerning social and personality characteristics of children. Horrocks & Buker (59) extended to younger children the earlier work of Thompson and his colleagues (140) on the constancy of friendships in children by obtaining the names of best friends at successive periods. There is no sex difference in the stability of friendship in these children, though wide individual differences in stability exist in any one age or sex group. Children become more stable in friendship in each successive age or grade group. The data conform to Thompson's results in the age levels which overlap. Tuddenham (147) used Tryon's nomination procedure (144) with 1500 children, sampling younger ages and a wider socioeconomic range. He finds that clear-cut patterns of characteristics associated with popularity are present as early as the first grade, though the clusters become more clearly separated and defined in each succeeding age group studied. In the first grade, variance in score is largely composed of "common" and "error" factors. Specific factors do not appear until the fifth grade. A sex separation in peer group social values shows as early as the third to fifth grade and becomes cumulatively important. The author concludes that

the problem of securing group approval for a boy is one of conforming to a clearly defined group of traits for which he may or may not possess the requisite strength or motor skill. For a girl the problem is more one of adapting to a continuously changing set of values which are never as clearly defined as they are for the boy.

The hypothesis that frequency of pregenital symptoms is associated with peer rejection was examined by Miller & Stine (102). One hundred sixty-six children, 7 to 14 years, were grouped regardless of age into five levels of acceptance by peers. Isolates constituted a sixth group. The number of unresolved pregenital symptoms revealed in the endings supplied to nine incomplete stories was tallied. Symptoms included instances of orality (food, touch), anality (smearing, cleanliness, money), ambivalence, magical thinking, frustration, extrapunitiveness, intropunitiveness, passivity, and leaving the field. Despite weaknesses in design, the authors affirm that the more the child uses pregenital modes of relating to others, the more he is rejected by his peers.

Forlano & Wrightstone (34) found that sociometric acceptance scores correlated $+.40$ or more with 40 items of the California Test of Personality, and that ratings of poor adjustment correlated $+.71$ with peer rejection. In general, a sociometric rejection score is more diagnostic of maladjustment than an acceptance score. Singer (128), however, finds a correlation of only $+.20$ between acceptance and the California Test of Personality with junior high children. Of significance is his observation that, despite much counseling, guidance, and actual training in skills given several less accepted chil-

dren during the course of a year, the pattern of group structure changed very little. Singer concludes that social patterns are set in the primary grades.

ATTITUDES

Landreth & Platt (74) demonstrate a factorial design to test effects of age, race, sex, social class, and area of residence upon attitudes toward skin color. The test device consists of 18 picture boards, with human figure inserts, attractively colored. Hartley & Schwartz (52) use a picture device to examine attitudes toward religious groups. They avoid caricaturing the groups by relying on pictured differences in diet, religious symbols, and home furnishings, and find that young school children recognize and correctly interpret the differences portrayed. Radke-Yarrow *et al.*, in a study already mentioned (112) observe that, since parents invest the school with considerable "authority" and accept it as legitimate common ground for children from different groups, the school probably should be the focus of efforts to improve intergroup relations. Chyatte *et al.* (23), by asking questions about attitudes toward Negroes and Jews in informal conversations, found that southern boys make more prejudiced statements than do northern boys of comparable social status. No age differences appeared in the southern group, but, contrary to many published reports, older northern boys showed less prejudice than younger subjects. Possibly the authors' direct question method may account for this. Johnson's study (63) of Spanish-American and Anglo children in the same town, using a projective test of attitudes, shows an increase of prejudice with age, and an increased correlation between the measure of prejudice and the extent of bilinguality in the older age groups. That children's intergroup attitudes may be influenced by cultural aspects which are intriguing and strange, as well as by familiar aspects, is demonstrated by Zeligs' analysis (160) of children's compositions describing the nationalities they would choose to be if they could not be American.

INTERESTS

A study of children's TAT² responses made in 1943 was repeated in 1950 (just prior to the Korean conflict) by Rautman & Brower (113). Percentage of themes on war decreased from 6.5 per cent to 1.8 per cent. Percentage of themes having to do with killing and death remained nearly identical. In 1943, 35 per cent of the stories had a happy ending and in 1950, 51 per cent. In 1943, 25 per cent had unhappy outcomes and in 1950, 15 per cent. Each study is based on approximately 5000 protocols; so these shifts in percentages are meaningful.

The development of interest patterns in young children is the subject of Tyler's first report of a longitudinal study (148). Four types of activities were investigated in 115 first grade children: active outdoor play; indoor play with toys; paper, pencil, crayon activity; and helping adults with work. Results indicate that work and play activities are negatively correlated, and that helping around the house is definitely a girl's activity. Two correlated factors account for most of the variance on a "Guess Who" measure. One of

these has to do with activity (noisy, aggressive, domineering) and the other with appearance (good looking—unkempt). Correlations between interests and reputation ratings, and between interests and Primary Mental Ability scores are insignificant.

Children's preferences in reading, movies, and radio programs were investigated by Lyness (80) in a large sample study. Age and sex trends for specific types of preferences are given in considerable detail. The author hypothesizes that three general patterns of interest exist: adventure and violence; love, private life and glamour; and educational interest. Age and sex differences familiar to most child psychologists are confirmed in this study. Of particular interest is the fact that preferences are shown to be reasonably consistent from medium to medium. This consistency is apparently greater for younger children than for older children, possibly because of the greater specificity and diversity of interest found among older children. Ricciuti's (115) study of listeners and nonlisteners to radio programs was based on questionnaires answered by 3000 children from the fifth to the eighth grade. He lists the percentage of regular listeners for different kinds of programs, time devoted to listening, and other results. Frequent listeners and nonlisteners are identified and contrasted on a variety of personality and achievement measures. Results are offered for all age and sex groups. Differences are more numerous in seventh and eighth grade boys, and in sixth and seventh grade girls than at other ages. The greater number of differences at these particular grade levels suggests an effect of puberal shift in interest pattern and of increased developmental heterogeneity at those age levels. Differences between frequent listeners and nonlisteners are found mostly in school performance records, not in personality data, and favor the listeners over the nonlisteners. There were very few differences in number of fears or nervous habits and insignificant differences on the Picture Frustration Test and an imagination test. Actually, only 89 of more than 1500 critical ratios listed are significant at the 5 per cent level; 75 such differences would be expected according to probability theory. Obviously, extensive or restricted radio listening has small impact on personality and achievement variables, when considered in a population of children. The cinema is a powerful stimulus to and supplies the content of children's fantasy, according to Wall & Simson's study (151) of British adolescents. Their findings concerning influence on the social behavior and attitudes of adolescents fit the reports from the Payne Fund studies made two decades ago in this country.

A study of children's religious knowledge by Thomas (138) reveals a widespread neglect of religious education in Catholic homes. Rural children receive more training than urban children, and regional differences appear, traceable mainly to ethnic variation.

PERSONALITY TESTS FOR CHILDREN

Of about 24 published papers dealing with personality tests for children, only five report experimentation with ratings or paper-pencil scales; the

others refer to various projective or expressive tests. Sister Mary Amatora (145, 146) has developed a rating scale for children suitable for self-ratings or ratings by associates. Two personality tests suitable for adolescents and adults are reported by Gough and his colleagues (42, 43). Both scales, one for dominance and one for social responsibility, are composed of true-false items, including many self-descriptive statements. Harris and others (51) have adapted this responsibility scale to children and find no age trends of mean scores from the fourth grade through high school.

There is a steady flow of Rorschach studies on children. Only those which report age trends or cultural influences will be mentioned here. Meyer & Thompson (99), Ledwith (76), and Carlson (18) report "normative" studies on groups of five-, six-, and eight-year-olds, respectively, totalling only 346 children. A somewhat more ambitious attempt is contained in the text by Ames and her colleagues (6), which describes Rorschach responses of children aged 2 to 10. Using samples of 50 children at each year of age, the authors offer median and Q values for various determinants at each age. An interesting picture of growth results, supplemented by highly descriptive qualitative statements about each age group. These create the impression of a progression of hypostatized patterns, with little recognition of the considerable overlap of adjacent age groups, which all measurement studies show. It is quite apparent that for the authors these age patterns are very real, that a description at age five and a half which recurs in very similar wording at eight represents a behavior pattern which actually skips year seven. The normative value of the data is marred by the fact that the samples are not representative.

Applying the Rorschach to more than a thousand Indian children of school age in 11 communities and 5 tribes, Thompson (142) finds marked individual differences in how children approach reality. Each tribe shows a characteristic pattern of perception which is clear-cut in all ages and both sexes.

With the Rosenzweig Picture Frustration test, Spache (129) found that child-child and child-adult conflict situations are handled somewhat differently by boys and girls and that more modal answers appear than test norms report. He suggests that more adequate standardization is needed. Walton and others (153) report a new picture-story test designed for children 8 to 14 years of age. Four pictures were found successful in differentiating at the 5 per cent level between 15 well-adjusted and 15 poorly-adjusted children as judged by a simple rating scale.

Normative data for the House-Tree-Person drawing test are supplied by Jolles (65), who limits his attention to the sex of the human figure drawn. About four-fifths of his subjects draw their own sex. Girls draw an opposite sex figure more often than boys do. Younger boys draw relatively more females than older boys, possibly reflecting their greater contact and identification with women. Ambivalent figures (sex not clear) are drawn by only one to 3 per cent; this is an exceedingly low percentage for young children. Stora (135), who compared handwriting methods, the drawing test of Prud-

hommeau, the Rorschach, and material obtained by interview of the child's parents, found a high concordance among the various methods.

The Wartegg drawing completion test widely used in Europe is just now becoming popular here. The simplicity of the test and the variety of results it evokes make it popular with clinicians. Mader's (83) excellent study is unfortunately relatively unavailable; so her experimental findings that the test measures drawing ability primarily (at least among adolescents), and not personality, may go unheeded.

PERSONALITY ORGANIZATION

An interesting problem of definition appears in a paper by A. J. Harris (50). He submitted 24 "normal" children (white boys, native born, at grade for age, middle range of SES,² average intelligence and school achievement, and making an average adjustment in the classroom according to the teacher), four from each of the elementary grades, to four child-study clinics. The clinicians who studied these children found many adjustment problems, some of which they considered very serious. Do clinicians get out of touch with pragmatic criteria of adjustment, or do we need more complete studies of how psychologically crippled people nevertheless manage to function at an acceptable level? Certainly, the study reminds us that clinical definitions of "normal" and "maladjusted" need constant examination with reference to statistical and pragmatic concepts of "average" and "deviate."

Walter & Marzolf (152) experimentally related sex, age, and school achievement to level of aspiration, using the Rotter aspiration board. Boys have higher goal discrepancy scores than girls, although girls are more variable than boys. While the older children exhibit higher goal discrepancy than younger children, the interaction of sex and grade is insignificant. No relationship between level of scholastic attainment and goal discrepancy appeared. The author interprets the sex difference as being due to the differentiating effects of adult and peer cultures to which children are exposed.

Asthmatic children carefully matched with nonasthmatics and trained in a level of aspiration problem set higher goals for themselves than did the controls, according to Little & Cohen (79). The mothers of asthmatics (who acted as recorders and who made private predictions for their youngsters as well) set much higher goals for their children than the rather reasonable goals set by mothers of control subjects. The mothers of asthmatic children did not differ very much from their children in the unrealistic goals they set, but the mothers of controls did differ from their own children in setting goals, being more realistic than their children. These differences persisted throughout the several trials of the experiment.

Level of aspiration is closely related to the self-image. Although there has been much concern with the self concept recently, few research papers have attacked this problem in children. Jersild (62) considers that self-understanding is an important aspect of personality organization in children. Every child is a practicing child psychologist, forming attitudes and evaluations of others and of himself; these concepts and attitudes are woven into

his life. This process, Jersild believes, should be a planned feature of education. Pointing out that there is a tremendous carry over of unhealthy attitudes into adult life, Jersild asks for much new research on how self-attitudes develop, and how particular experiences influence these attitudes. He cites the need to know at what levels children can develop "psychological insight" into the behavior of other persons and into their own behavior.

A developmental description of the self-concept by Sarbin (121) notes five stages in the first two years of life: (a) the self-concept is fixed at the somatic level up to a month of age; (b) receptors and effectors increasingly come into play to constitute a second phase up to approximately six months of age; (c) between six months and approximately one year, there is the "primitive-construed self"; (d) between 12 months and 20 to 24 months the introjecting-extrojecting self appears; (e) after his second birthday, the child presumably becomes capable of adopting social roles, and the true social self emerges. This description of the development of the self could be strengthened by empirical evidence from the child development literature. There is little reference to self-awareness as reflected in the language development in children, and no recognition of the imitative, "role-playing" activity which many children exhibit as early as 16 or 17 months.

Adolescence may be a time of re-evaluation of the self. Kitamura (71) asked a group of boys to rate their satisfaction with their disposition (temperament), and their facial appearance, on seven-point scales. Dissatisfaction with disposition increases sharply in mid-adolescence. Since 15-year-olds in middle school give different results from 15-year-olds in high school, and since the shift in dissatisfaction occurs at about age 15 when the school transition is made, Kitamura insists that this change in the self concept is related more to the social setting than to age. Dissatisfaction with appearance is highest in midadolescence (high school). Sex-inappropriate facial features frequently appear as sources of discontent.

Taylor & Combs (137) show that well-adjusted more frequently than poorly adjusted children, as indicated by the California Test of Personality, assent to statements which, while probably true of all children, could be considered threatening, for example: "I sometimes disobey my parents," and "I sometimes use bad words." Citing the similarity of their items to many paper-pencil personality tests, the authors question the validity of such tests, wondering whether well-adjusted children may not be underestimated by them. Contrary to this hypothesis is the evidence of many studies, using a variety of criteria of selection, that poorly adjusted children are more likely than well-adjusted children to subscribe to statements with socially undesirable content.

Two papers discussing medical and psychological aspects of pseudohermaphroditism are of interest to child psychologists because of the full descriptions of the self-attitudes of children reared as girls to adolescence and the shifting to the masculine role. Both Chapman *et al.* (22), and Parnitzke (108) report that anomalous psychological reactions, rather than lack of appropriate physical development, finally sent their subjects to a

physician. Parnitzke traces the child's readjustment in the school class through a series of sociometric ratings for a three-month period. In each case, the changed self-image was achieved quite readily and brought considerable relief to the troubled adolescent, though neither subject had had an unusual or unhappy childhood in the girl's role.

In order to determine how reactions to frustration may be related to parental domination, Kates (69) utilized the Picture Frustration Test, a suggestibility test, and four incomplete stories, each with a different central figure as a frustrating agent. On the basis of each story completion, children were classified as submitting to, rejecting, or being ambivalent toward the mother, father, chum, or peer group leader. At the 5 per cent level of significance, the children who submit to the mother are significantly more expunitive than those who reject the mother. The latter were significantly more intrapunitive than the former. No differences in impunitiveness were noted. Those who reject the neighborhood leader and their chum are both more expunitive and less impunitive than those who submit. No differences in intrapuniveness were noted. The less suggestible subjects were thought to be significantly more expunitive and less impunitive than the more suggestible subjects. No reliabilities are given; because the single test item is used as a criterion, caution in the acceptance of these findings is advisable.

An experimental study of frustration, using a paper-pencil test to induce thwarting, is considered by Sherman & Bell (127) to have mental hygiene implications. Although frustration was not extreme, the efficiency of many children was impaired thereby. The more efficient suffered less than those less able to cope with the material.

Graham and colleagues (44) investigated aggression in adolescence, utilizing 50 incomplete aggressive statements which took the form "When John's mother hit him he ____" and included other forms of provocation, and other offending agents. The frequency and degree of aggressiveness of the responses were direct functions of the punishment-threatening value of the attacking person; the greater the potential threat, the less the aggression in the response. No evidences of displacement or compensation were found. The person relatively more aggressive towards inferiors was also relatively more aggressive toward parent figures. No social class differences in aggression appeared.

Aggressiveness as well as popularity have been found related to accident-proneness in children. Fuller & Baume (39) report that classroom accidents in the third grade correlate significantly with records of the previous year. Boys received a higher mean number of injuries than girls. Regardless of sex, the high injury group got distinctly poorer scores on the Haggerty-Olson-Wickman rating scale (more active, restless, aggressive) and lower happiness indices on the Fuller Emotional Index Scale. Sociometric analysis of the third grade children shows that the less popular children have more accidents, and more of these accidents are of a social than of a nonsocial type.

Children's art has long been regarded as a source of information on personality organization. Mildred Martin (87) applied the criteria developed by Elkish (32) to finger paintings of 10 poorly adjusted and 10 well-adjusted preschool children. The well-adjusted demonstrated a greater sense of freedom, better integration of behavior, and a more realistic approach to life situations. Thomas (139) attempted to evaluate certain of the claims by Alschuler & Hattwick (4) concerning the significance of color, line, and form in children's paintings. Experimental and control groups of nursery school children produced paintings following nonfrustrating play. Results in general fail to support the Alschuler & Hattwick contentions. There was no significant change in the use of color, line, or form following the frustrating play; nor was there evidence that temporary frustration induced any regression in painting behavior. As in the preceding studies, Martin & Damrin (89) find that children's drawings and paintings can be rated according to certain criteria with a considerable degree of consistency. Mean ratings for a number of qualities such as symmetry and expansiveness were intercorrelated for a population of 31 drawings. Three factors are found to account for most of the variance in the characteristics attributed to the drawings by the raters.³

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³ More than 30 excellent quantitative studies of personality disorganization in children appeared this year but cannot be reviewed here for lack of space. Likewise the literature utilizing the case approach has been omitted. A striking feature of this review is the dearth of material on motor and intellectual growth and the development of skills as contrasted with the emphasis on social influences on personality.

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LEARNING^{1,2}

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Within the year covered by this review, well over 200 papers were published dealing rather directly with learning. Clearly, within the space allotted, no meaningful summary of all these is feasible. While the possibility of unrecognized biases in selecting papers for this review cannot be denied, an effort has been made to choose papers on the basis of the following criteria: The paper must have (a) methodological importance for future research; (b) bearing on extant theoretical issues; (c) resulted in new systematic laws or new phenomena; or (d) contributed new theoretical concepts. Finally, some papers are discussed in which, according to the reviewer's opinion, the facts are misinterpreted or theoretical concepts are inappropriately used.

Sooner or later most students of learning make statements about the pervasiveness of learning. It is commonly said that the learning process underlies most of our affective, perceptual, motivational, and motor behavior. The reviewer's agreement with this belief can best be shown by a brief documentation of the permeating nature of learning from the past year's work. Worchel & Mauney (100) demonstrated marked improvement in the perception of obstacles by the blind under appropriate practice conditions. Leuba & Dunlap (50) had hypnotized Ss³ concentrate on two sensory phenomena, e.g., sound of bell and pinprick to hand. When awakened (with amnesia for the hypnotic state), S complained of a pain in the arm when asked to imagine a bell ringing (conditioned imagery). Notterman, Schoenfeld & Bersh (72) paired a tone and a shock in a conditioning procedure and measured the change in heart rate. Conditioning was evident in the depressed heart rate to the sound of the tone alone. Extinction and spontaneous recovery were also observed. The role of experience in the development of perceptual-motor habits in the chimpanzee was investigated by Nissen, Chow & Semmes (70). From the age of 4 weeks to 31 months the four limbs were kept encased in cardboard cylinders. Following removal of the cylinders certain functions, e.g., learning which hand was stimulated, was markedly retarded, while others, such as perception of size and form, were little impaired by the lack of opportunity for differential tactual and kinesthetic experience. Ullman (92)

¹ This review covers journals received to May 1, 1952.

² The following abbreviations and symbols are used in this chapter: Ss (subjects); CR (conditioned response); CS (conditioned stimulus); US (unconditioned stimulus); I_R (inhibitory potential); sI_R (conditioned inhibition).

³ I am indebted to members of my graduate seminar (James Freeman, Robert Gardiner, Winfred Hill, and Sarnoff Mednick) for their discussions with me of many of the papers reviewed.

established a compulsive eating habit in rats and Gelber (26) demonstrated learning in the *Paramecium aurelis*. The permanence of learning is shown by Smith (80) in her attempts to recall material neither seen nor rehearsed for 16 years and originally learned 40 years previously. A loss of only a few percentage points was evident.

The diversity of phenomena interpreted as resulting from learning is evident from the above extreme illustrations. But the steady progress of the science of learning is not to be measured by illustrations of the encompassing nature of the process but by work in areas with established methodology and some theoretical orientation. It is to this work, which constitutes the bulk of the year's energies, that the remainder of the review is devoted.

MOTIVATIONAL AND REINFORCEMENT PROBLEMS

Basic drives.—Within certain limits, deprivation time and activity level of rats are related directly. A study by Finger (22) on this matter has far reaching methodological implications. After some animals had been deprived of food for 24 hr. and others for 72 hr., they were satiated and activity level was measured on subsequent days under standard feeding schedules. Following satiation, activity level dropped for the 24-hr. group but was back to normal within a day or two. But the 72-hr. group showed marked depression in activity following satiation and did not show complete recovery within two weeks. Additional relationships pertaining to this "satiation syndrome" must be worked out, such as filling in the hours between 24 and 72, examining the thirst drive under similar conditions, and determining the relationship between learning and the prolonged depression in activity following satiation. Nevertheless, it is clear that the implications of Finger's findings must be carefully weighed when designing future learning experiments which call for deprivation-satiation conditions.

Work continues on a relatively new conception of drive in human Ss. Called "anxiety," this drive is measured by responses to selected items of the Minnesota Multiphasic Personality Inventory (87). The common procedure is to select Ss having low anxiety ratings and others having high anxiety ratings, subject them to a standardized learning situation, and measure the performance differences. Whether or not the anxiety referred to is of the same basic nature as that discussed by clinicians is for research to decide; on definitional grounds the anxiety is operationally clear. The usefulness of the concept will rest on predictions which can be made from postulated characteristics of the relationship between drive and response strength. These predictions, and the assumptions on which they are based, have not yet appeared. It is apparent, however, that predictions of differences in performance between anxious and nonanxious Ss will depend upon the nature of the task. Whether drive (as defined by anxiety) influences associative strength or merely response strength has not yet been experimentally determined. Spence & Taylor (86) have shown that percentage of CRs² given

by anxious Ss in eyelid conditioning is greater than the frequency produced by nonanxious Ss, but, as the authors point out, there is no way to tell whether or not this indicates different associative strengths.

Hilgard, Jones & Kaplan (37) have studied the influence of anxiety on discrimination training in eyelid conditioning. It was hypothesized that anxious people react more to their own apprehensions than they do to the objective environment. Following simple conditioning, the discrimination situation was introduced in which the CS² (change in light intensity) to the right side was always followed by the US² (air puff) but the identical stimulus to the left was never followed by US. Frequency of CRs to the negative stimulus was greater for anxious than for nonanxious Ss, indicating poorer discrimination. This is a relatively simple situation, and it is not easy to see how such results could obtain. Nevertheless, it is possible that the similarity of the positive and negative stimulus situations was such as to provide interference. If such is the case, an additional assumption is needed, namely, that interference is greater for anxious Ss than for nonanxious. Furthermore, since in many situations interference can be traced back to generalization tendencies, it is quite possible that the basic difference between anxious and non-anxious Ss lies in the difference in breadth of the generalization gradient, with the breadth being greater for anxious Ss. Anxiety, it will be remembered, may be thought of as a drive. There is some evidence [Brown (10); Rosenbaum (75)] that generalization gradients are broader under strong drives than under weak drives. If this is the case it can be predicted that anxious Ss will perform better than nonanxious Ss in situations in which there are few generalizing tendencies and poorer in situations in which there are many.

Learning drives and rewards.—Miller's technique (62) has become fairly standard for investigating acquired drives. An animal is shocked in one compartment but is allowed to escape to an adjacent one. After a small number of trials the shock is discontinued but the rat still persists in running to the safe compartment. This persistence in running is taken as evidence of acquired drive. Furthermore, the animal will learn an instrumental response (spinning a wheel or pressing a bar) with apparently only the acquired drive serving as energizer. According to Miller's interpretation, the original running response is strengthened because escape from shock is a reward in that pain stimuli cease. In addition, the stimuli of the shock box are quickly conditioned to elicit fear of shock and it is the fear which has the status of an acquired or secondary drive. Shock or no shock, the fear is reduced when the animal reaches the safe compartment. While Miller places primary emphasis on acquired drive in the above situation it is by no means easy to dismiss entirely the incentive function of secondary rewards. In this situation any stimuli which are present when pain ceases should gain secondary rewarding power. Since Hull (41) allows primary rewards to have incentive function (adding to the total drive state) there is no reason why secondary rewards may not also have certain drive functions. Miller's situation does not allow

a factoring of the relative contribution of secondary rewards and secondary drives to the total drive state.

A new technique for measuring development of a learned fear drive has been developed by Brown, Kalish & Farber (11). They reasoned that during development of conditioned fear other response tendencies should be intensified if those responses are not incompatible with the fear response itself. The response chosen was the startle response. By an ingenious arrangement the magnitude of the startle response of rats to a toy cap pistol was measured by a delicate postage scale, the amount of scale depression being transmitted to an automatic recording system. During training a light-buzzer combination was CS with shock as US. On every third trial the startle stimulus was presented instead of shock. In a control group the US and CS were also presented but never contiguously, and the pistol was fired every third trial. The results show that magnitude of startle response increased as a negatively accelerated function of training trials in the experimental group but showed little change in the controls. Extinction and spontaneous recovery of the startle response, hence of the fear drive, were also demonstrated. As mentioned above, a conditioned fear will probably facilitate other responses only if they are not incompatible with the fear response. If they are incompatible, fear may inhibit them. Libby (52) showed that conditioned fear has a depressant effect on a well-established lever-pressing habit and that as number of pairings of CS and US (shock) increases, the depressant effect of fear first increases and then decreases. Gwinn (32) has found suggestions of the same relationship, but its interpretation is not clear.

Secondary rewards have been reported by enough investigators that they are presumed to be reliable phenomena. Any neutral stimulus which occurs consistently with a primary reward may itself perpetuate behavior originally produced only by the primary reward. Thus, extinction of a response should be prolonged for a greater period of time if a secondary reinforcer is present than if one is not present. Seeman & Kjenaas (78) have failed to find such an influence on extinction by stimuli which had been previously paired with primary rewards. This was true for either hunger or thirst and their relevant primary rewards. The reasons for failure to find secondary rewarding effects are not clear, and since the study is reported briefly it is difficult to suggest possibilities. One point may be mentioned, namely, that only eight training trials were given and this may not be enough to establish the association between the neutral cue and reward. The authors suggest only that clarification is needed in specifying the manner in which secondary rewards may be demonstrated, i.e., extinction may not be an adequate index. On the other hand, Hall (35) apparently experienced no difficulty in showing a fairly direct relationship between number of pairings of the neutral stimulus and primary reward and the effects of the neutral stimulus in a new situation. The fewest number of pairings used was 25. Likewise, Notterman (71) clearly showed the effect of secondary reinforcers. In his experiment the number of nonreinforced trials varied (on which the neutral stimulus was also omitted)

while the number of reinforced trials remained constant. Under these conditions the effect of the secondary reinforcer was directly related to number of nonreinforced trials. This suggests, as does also a study by Ferster (21), that the most effective secondary reinforcers will be established if during training the response is sometimes allowed to occur in the absence of the neutral stimulus and no reward follows the response.

Irrelevant drives and latent learning.—More than one drive may be operative when a response is learned, yet only one may be rewarded. Does the irrelevant drive—the nonrewarded drive—function in any systematic way to enhance or retard performance? Are the drive stimuli of two drive states, especially hunger and thirst, independent? In an earlier study Kendler (46) advanced the hypothesis of selective association of drive stimuli. In his study rats, while both hungry and thirsty, ran a T-maze in one arm of which they found food and in the other water. When made either hungry or thirsty, they chose the arm which led to the relevant reward. These results, suggesting the idea of selective association, ran counter to Hull's conception (40) that any stimuli present at the time a response is rewarded became instigators for that response. Eninger (19) took another line of attack on this problem. If the selective association hypothesis is adequate, he reasoned, and if irrelevant drive stimuli are present during the acquisition and also during extinction, extinction should be no slower than for a group with different irrelevant drive stimuli during extinction. On the other hand, if drive stimuli do become associated with a response based on a different drive, extinction should be slower where irrelevant drive stimuli during learning and extinction are the same. Animals were first trained to escape shock in a T-maze, the correct goal box being the one chosen on the first trial. During 50 shock trials half the animals were under thirst drive and half under hunger. Since neither food nor water was found in the maze, these drives would be classed as irrelevant. But, escape from shock being a reward, it might also be expected that the escape response would be attached to irrelevant drive stimuli if the selective association hypothesis is incorrect. After training, shock was discontinued and extinction began. Half the animals trained under thirst now had thirst during extinction and half had hunger; the same was true for the group trained under hunger. The results clearly show that animals trained and extinguished under the same irrelevant drive (whether hunger or thirst) took longer to extinguish than those trained and extinguished under different drives. Thus, these results are contrary to the expectation of selective association. Eninger concludes that irrelevant drive stimuli do become associated to overt responses but that response strengths may be of lower magnitude than those to relevant drive stimuli. Kendler's results could be explained by such an hypothesis and the hypothesis is quite compatible with one proposed by Walker (95). Walker suggests that certain drive stimuli of hunger and thirst are common to both states, while others are independent. If this is the case, it would be expected, as found by Eninger, that irrelevant drive stimuli are associated with responses leading to relevant reward but

that the association is of less magnitude than an association based on relevant drive.

Latent learning studies are concerned with the role of rewards in learning. Such a simple statement, however, does not adequately describe the current trends in methodology used to attack the problems. It is more accurate to describe these studies as asking the question: Does an animal learn anything about the location of a reward when the drive relevant for that reward is satiated? Even to the casual student of learning, "latent learning" has come to mean the studies which will provide a "crucial" test of antithetical postulates of two prominent contemporary theories of learning, namely, Hull's reinforcement theory (40, 41) and Tolman's sign-gestalt or perceptual-learning theory (90), this latter theory, of course, predicting that learning may take place without reward and pointing to latent learning as evidence. With several score studies already reported we are still without a clear-cut evaluation of the postulates and, furthermore, have very few behavioral laws to show for the great amount of experimental work. It has often been said (to the point of becoming a near-hackneyed expression) that theoretical controversy is good because it spawns research. But when theoretical controversy produces research which does little to clarify theory and produces no systematic behavioral laws, the value of operating on the platitude may be questioned and our philosophy of experimental science might stand re-evaluation. Research on latent learning has become largely a control-experimental, yes or no, type of research. Since the acceptance of a "yes" or "no" answer appears to depend somewhat upon one's theoretical bias, the student of learning without strong bias is left with little. If an experiment represents a systematic manipulation of a variable, at least one has a behavioral law, although the experiment may not have turned out to be the crucial test of latent learning which the experimenter thought it would.

If we wish to know whether there is or is not latent learning, and seek the answer only in the past year's work, we must conclude that some investigators report it and others do not. The work has, to a large extent, been patterned after the original Spence-Lippitt (84) experiment. The basic procedure is to train animals in a simple T-maze with say, a thirst drive, but with hunger satiated. The animal gets water if he takes the appropriate alley; whereas if he takes the other he will merely find food (which he doesn't eat because of satiation). The question concerns whether or not the animal learns the location of food so that when he is made hungry he will make the appropriate response. If he does make the appropriate response he is said to have shown latent learning since the learning has taken place without any consumatory response (reinforcement by primary reward). One obvious question is, of course, whether or not the animal can discriminate between drive stimuli. The evidence is fairly conclusive on this matter. For example, Rethlingshafer, Eshenbach & Stone (73) found they could train animals to take one alley when the animals were thirsty and a different one when hungry. Indeed, as one of my students suggested, if an animal cannot discriminate

between drive stimuli it might drink when hungry and eat when thirsty. Christie (14), however, recognizing how critical drive discrimination is to the Spence-Lippitt type studies, felt that a test should be made directly in the latent learning context. He found no latent learning but did find evidence that animals were discriminating between drives and concludes that inability to discriminate drives cannot be used as an argument for failures to find latent learning.

In latent learning experiments it is possible that sight of food might serve as a secondary reinforcer even to a satiated animal. If this were true, latent learning (if it occurred) in the Spence-Lippitt type experiment might be accounted for on this basis by a reinforcement theorist. An adequate test requires that there be no competing drive which is primarily reinforced. Thistlethwaite (88) made a test of this proposition. Animals, satiated for food but thirsty, were given series of free runs in a Y-maze, with food on only one side and no incentive on the other. Over a period of 20 days he found a slight tendency for the animals to run away from the side on which food was located (this may be related to "switching" behavior to be discussed later). There is no evidence during the free runs that the food had any "drawing power" for the satiated animals. Interspersed with the training days were test days on which the animals were hungry but on which no food was present in the maze. On these test trials there was a distinct tendency for animals to go to the food side more frequently than on training trials. A control group showed, however, that this difference was significant on only the last of five test trials. There is, then, some evidence for learning which is interpreted as being unaccounted for by secondary reinforcing effects of food and is attributed instead to perceptual learning of the Tolman variety. To show the further disagreement in the findings, we may only mention that Fehrer (20) also found some evidence for latent learning while Denny & Davis (17) did not. Thus, we have cases in which latent learning was found but its magnitude does not compare with learning which occurs with primary reward. Other studies have importance for latent learning problems. Kendler & Levine (48) have shown that if animals are trained to go down one alley for dry food (while satiated for water) they will tend to take the opposite alley when drives are reversed. This phenomenon ("switching") may account for alleged latent learning reported in earlier studies but may represent only an avoidance of dry food and not an approach to water.

MacCorquodale & Meehl (58) have demonstrated convincingly that, given absolute freedom in a multiple-unit maze, animals will learn to avoid narrow blind alleys. This is interpreted as resulting from the "negatively reinforcing aspect of having to turn around in a confined space" (p. 371). Montgomery (65, 66) has presented a fairly independent set of operations for defining an exploratory drive in rats. Rats placed in a multiple-unit maze will go from one section of the maze to another (avoid repetition), this tendency decreasing with exploration and spontaneously recovering with rest. Now, earlier experiments on latent learning had used a technique of

giving the animal unrewarded trials in a maze or allowing the rat to spend several hours exploring the maze. Then, when food was introduced, the animal showed error reduction of such magnitude that it was considered learning had taken place on nonrewarded trials. Reinforcement theorists have been hard pressed to explain such results. It is now possible for such a theorist to give an explanation which may be adequate. Accepting the validity of exploratory drive (but not necessarily accepting Montgomery's idea that the drive is reduced by exploring), and accepting the fact that animals learn to avoid cul-de-sacs due to negative reinforcement, the path from the starting box to the goal box would be the strongest series of responses. Then, once an incentive is introduced (providing motivation to run), acquisition will appear to take place very rapidly (latent learning). The drop in errors upon the introduction of the incentive would be further enhanced because there would be a tendency for the animals to avoid blinds pointing away from the goal. There are obvious tests for such an explanation. For example, making cul-de-sacs very wide or of such nature that negative reinforcement would be improbable should reduce the amount of latent learning. However, it seems likely that it will be difficult to demonstrate that there is no true latent learning in such situations. Rather, it would seem that a conclusion could be reached that latent learning, if it exists, is of very minor importance in the rat's behavior as compared with learning by reward. This is the same conclusion that can be reached at the present time.

Avoidance conditioning.—One of the more plaguing problems for drive-reduction theorists is the interpretation of conditioning when shock is US. In one set of operations the occurrence of CR results in avoidance of shock (avoidance conditioning) and in the other it does not (nonavoidance). In avoidance training, when CR occurs and shock is avoided, what is the reinforcement? Currently this is accounted for on the basis that the CS, being always followed by shock, becomes a fear cue and its cessation fear reducing (secondarily reinforcing). Nevertheless, each time a CR does avoid shock the secondary reinforcing properties of cessation of CS might be extinguished somewhat. On the other hand, in the nonavoidance procedure, primary reinforcement is present on every trial. It should, therefore, be predicted by reinforcement theorists that, unless complicated by other interfering CRs which may be inadvertently conditioned, nonavoidance conditioning should be superior to avoidance conditioning. Logan (55) has shown this to be the case for human eyelid conditioning. The importance of other interfering responses in making predictions concerning avoidance and nonavoidance conditioning is clearly shown by Gibson (28). Goats were conditioned in a situation in which there was practically no restriction on the animal's movements; the animal could move around in the room and yet the CS and US (shock) could be delivered at any time. Gibson's results show that in nonavoidance procedure animals consistently shifted from one type of response to another as if in continuous trial and error attempts to avoid or diminish effects of shock. In avoidance conditioning, on the other hand, animals soon

learned to fixate the response (foreleg flexion) which avoided the shock. Concerning the nonavoidance animals, Gibson reasoned that shock tended to suppress motor behavior which immediately preceded it so that on subsequent trials another response appeared. But, since the great variability of response occurs to CS and not to US, Gibson concludes that there is an emotional conditioning which develops early in the procedure. Thus, shock has two functions in the nonavoidance condition; it reinforces an emotional reaction and depresses motor action which preceded it. In avoidance conditioning only the first function is operative. The Gibson study is an important one, for it demonstrates clearly, when taken in conjunction with Logan's results, that the prediction one makes about learning with and without avoidance of shock depends heavily upon the nature of the situation. When many responses are available and one of these is "right" by experimental dictum, avoidance conditioning of that response is much superior to nonavoidance; when there is a very restricted number of responses which can be made (as in eyelid conditioning), the nonavoidance is superior.

Partial reinforcement.—During acquisition a CR may be reinforced from zero to 100 per cent of the time. It is known that rate of learning is a function of this variable, with the greater the proportion of reinforced trials, the more rapid the learning [Jenkins & Stanley (45)], although the function has not been carefully worked out. A study by Grant, Hake & Hornseth (30) has introduced this variable to what they choose to call "verbal conditioning." S is presented a board in which two light bulbs are inserted. He is instructed that when the left light comes on he is to guess whether or not the right one will come on. In this particular experiment five groups were used, with differences for the groups being the proportion of times the right light actually followed the left, the values being 0, 25, 50, 75, and 100 per cent of the times. Sixty trials were given at the end of which Ss were responding with a frequency almost exactly equal to the true frequency. For example, the 25 per cent group was guessing that the light would come on about once every four trials. It is not easy to specify what these data mean other than the fact that college students can translate frequency of a given event into predictive frequency. Since we know that a large proportion of students can think effectively in terms of probabilities [Morgan (67)], the above results may simply represent the ability to perceive the probability of an event and predict on that basis. However, Jarvik (44) reports, as a consequence of an experiment very similar to the above, that Ss could not as a group report in any articulate fashion the pattern of stimuli presented them during an acquisition series.

Somewhat more complex partial reinforcement conditions have been given by Hake & Grant (33) in eyelid conditioning and Hake, Grant and Hornseth (34) in verbal conditioning. Two variables were manipulated, namely, number of nonreinforced trials in a block and number of transitions from a reinforced block to a nonreinforced block. Each was manipulated three ways, and while the number of reinforced trials was the same for all

conditions, the number of nonreinforced trials varied from 2 to 30. To a certain extent, allowing the number of nonreinforced trials to vary confounds the results for the other variables. At least in eyelid conditioning, where evidence suggests that reactive inhibition develops [Spence & Norris (85)], we may suspect that such inhibition developed differentially for the conditions as a consequence of differences in the frequency of nonreinforced trials. If reactive inhibition is different for the conditions, it should be demonstrable in the acquisition data. Acquisition data are not given; results in terms of extinction show that no significant differences existed among the conditions. Nevertheless, it is quite possible that the effect of the manipulated variables (blocks and transitions) may have been counteracted by effects of differential reactive inhibition. In the reviewer's opinion, therefore, these experiments do not provide an adequate test of the influence of the two variables manipulated.

GENERALIZATION

Generalization continues to be used as an explanatory concept for a wide variety of phenomena. Paralleling this are further researches on the characteristics of generalization per se. The more precisely these characteristics are determined, of course, the more accurate becomes the use of generalization as an explanatory mechanism. This work, to a large extent, consists in a study of the generalization gradient for different kinds of tasks and materials. In general, those psychologists who use generalization as an explanatory tool accept it as a "given"; little attempt has been made to "explain" generalization. It is therefore an achievement to show a physiological correlate of it. Fink & Davis (23) demonstrated that nonovert muscular responses, as measured by electromyographic technique, show the same generalization gradient as overt muscular responses in a simple key-pressing task.

One of the more difficult problems for a reinforcement theorist to handle is sensory preconditioning. If two stimuli are paired together (without any apparent reinforcement) and if later one is used as CS, the other will also evoke the CR [Brogden (9)]. Although a possible explanation in a reinforcement framework has been offered [Spence (82)], no independent tests have been made of the explanation. Wickens & Briggs (98), however, guessed that sensory preconditioning was a case of mediated generalization, the mediation taking place via a common response made to the two stimuli during the association stage. They reasoned that if this were correct the same results (sensory preconditioning) would occur if the two stimuli were presented separately but a common response attached to both. In the experiment Ss in one group said the word "now" to the presentation of a tone and light together under the guise of a reaction-time experiment. Ss in another group had tone and light presented separately with the instructions to say "now" as soon as one or the other came on. Thus, in both groups a common response was being made to the two stimuli, but in the second case the two stimuli were never presented together. Following this initial stage, finger

withdrawal was conditioned to the tone and on the test trials only the light was presented. Appropriate controls were run which had only light or only tone during the original stages. The results show no difference in frequency of response for the two experimental groups on test trials, but the frequency was far greater than for the controls. Thus, "sensory preconditioning" was demonstrated even without the original pairing of the two stimuli, although the two may have been linked by the common response made to both. It is concluded that sensory preconditioning may be a case of mediated generalization.

REACTIVE INHIBITION

Animal learning.—Nearly all psychologists working in the field of learning accept the distinction between learning and performance. While performance is used to infer the existence of learning (habit strength, associative connection, etc.), the inferences do not admit of a one to one relationship between learning and performance. For it is commonly observed that performance may change considerably following certain conditions without further practice on a task (spontaneous recovery, disinhibition, and reminiscence). If learning and performance are not directly correlated, theorists have reasoned that there must be an inhibitory component which accrues with practice and which masks true learning to a certain extent. Therefore, performance measures at any moment may be constituted of learning factors and inhibitory factors. The most formalized inhibitory concept is Hull's reactive inhibition, now called "inhibitory potential" (I_R).²

Whenever a reaction is evoked from an organism there is left an increment of primary negative drive which inhibits to a degree according to its magnitude the reaction potential to that response (41, p. 74).

A further postulate assigns dissipation properties to I_R with rest. Thus, if conditions are such as to generate I_R , it is only following rest that performance approximates learning unconfounded by an inhibitory component. It becomes obvious, however, that one of the problems related to the formulation concerns the rate at which I_R dissipates. It is a problem because unless all I_R has dissipated, inferences concerning learning will be in error. On the other hand, if the rest interval is extended to "make sure" that I_R has dissipated, other factors (e.g., forgetting) may become operative. The danger of assigning generalized decay function to I_R is illustrated in the results of a study by Zeaman & House (101). These authors used alternation behavior in a T-maze to infer existence of I_R . Alternation behavior is shown when an animal, rewarded in both arms of a T-maze, tends to alternate from one arm to the other. This has been accounted for by I_R theory. For example, if a rat turns left on one trial, a certain amount of I_R is produced which would tend to reduce the likelihood of making the left turn on the next trial; consequently, if strength of association for the two arms is roughly equal, the animal will take the right arm. The theory would also predict that as time between

making a given response and the alternation test increased, frequency of alternation should decrease (I_R would dissipate). Zeaman & House showed that this was indeed true; as the time increased up to 24 hr., alternation decreased. Other studies have led to the conclusion that all I_R would decay in a matter of minutes rather than hours. With regard to interpreting alternation behavior on the basis of I_R , it must be pointed out that certain common sense principles should be considered. In the Zeaman & House study it was supposed that I_R decayed as a function of time with dissipation being complete by 24 hr. With any length interval, but especially with such long intervals, it is difficult to see that I_R can be so fantastically specific to the maze situation. Surely, in the time lapse between the training and test trials, while the rats were in the home cages, many turns were made, both right and left. And, these turns probably involved the same musculature as those in the maze. Why such behavior is so irrelevant (in theory) to maze behavior is difficult to understand.

Montgomery (64) has tested an additional proposition relative to adequacy of I_R as an explanatory concept for alternation behavior of the rat. If I_R dissipates as a function of time, the longer the time between successive trials, the less the likelihood of alternation. All animals were given 100 acquisition trials on a Y-maze; animals pressed a bar to obtain food and were reinforced in both arms of the maze. Five groups differed in terms of the inter-trial interval, namely, 10, 20, 30, 45, and 90 sec. For all 100 trials combined, alternation was shown to decrease as the interval between trials increased. For the course of the 100 trials, percentage alternation increased for the 10- and 20-sec. groups, but decreased for the other three longer-interval groups. No adequate accounting of this particular finding is given. However, Montgomery rejects I_R as an explanatory mechanism (on grounds which are not clear to the reviewer) and postulates "exploratory tendency" as a more profitable approach to explanation. In subsequent studies (65, 66) he has shown that exploratory tendencies do have the characteristics needed to account for alternation behavior.

In Hull's system, I_R is used as the mechanism producing experimental extinction. Each unreinforced evocation adds I_R but no habit strength so that eventually I_R will be of such magnitude as to produce complete cessation of responding. One of Hull's postulates relates amount of work and speed of extinction directly (41, p. 81). Thus, in a Skinner box, rats extinguished on a heavily weighted bar will extinguish more rapidly than on a lightly weighted bar. At least two studies have in the past confirmed this prediction [Mowrer & Jones (69); Solomon (81)]. Applezweig (5) has also confirmed this expectation and has ostensibly added a new principle, namely, "the more effortful the performance of a task, the better will it be learned" (p. 235). We do not believe that Applezweig's results can be accepted in support of this principle. It should be mentioned first that Applezweig analyzed the Mowrer-Jones study and the Solomon report and concluded that it was possible that different response potentials for the different degrees of effort may

have existed before extinction started. Clearly, this would confound the extinction scores and, if true, needs experimental clarification. Applezweig used a factorial design in which all combinations of five weights were used in acquisition and extinction, the weights being 10, 20, 30, 40, and 50 gm. in the lever-pressing situation. All animals were given 50 reinforced responses during training, but these had to be completed in two 30-min. periods or the animals were discarded. The number of animals eliminated was directly related to the size of the weight; 17 animals were eliminated from the 50-gm. group and 1 from the 10. It is apparent that a selection is produced. Those remaining in the large-weight groups must have been better learners or stronger (although not larger) than those remaining in the small-weight groups. Furthermore, Applezweig tallied partial responses during training, i.e., responses in which the lever was partway depressed but not enough to receive food. The frequency of these was directly related to weight size. Now, during extinction, with extinction weights held constant, number of extinction responses was directly related to size of weight during training—extinction was slowest with heaviest weights. It is from this fact that Applezweig infers that learning was greater with heavier weights. However, the two factors mentioned above could predict the same findings, each by itself. Finer selection of rats in the heavy-weight groups could produce the slower extinction. Furthermore, since the partial depressions would in effect produce partial reinforcement, and since partial reinforcement has consistently been shown to be followed by slower extinction than 100 per cent reinforcement, we would expect slower extinction for the heavy-weight groups. We must, therefore, reject this one conclusion. The study does show, however, that with varying training weights held constant, thus eliminating any possibilities of differential learning, extinction is faster the heavier the weights.

Human motor learning.—The state of work in motor learning since the war has been interpreted largely in the theoretical context provided by I_R as discussed above in animal learning. It has simply been assumed that a certain amount of I_R is generated with each response, thus providing a depressant effect on performance. And, I_R is assumed to dissipate rapidly with rest, the figure of 10 min. being commonly given as sufficient to dissipate even the maximum amount of I_R . In general some such hypothetical construct as I_R has been extremely useful in ordering data. Differences in effect of massed and distributed practice on performance can be readily handled as can reminiscence. It is wise to periodically remind ourselves that unlike the verbal learning situation, differences produced by massed and spaced practice in motor tasks (e.g., pursuit rotor, reversed alphabet printing, block turning) are enormous. For example, Duncan (18) shows that, when complete massing is compared with a distribution ratio of 10 sec. work to 20 sec. rest, distribution is superior to massing in spite of the fact that Ss under massing had three times as much practice as those working under spaced conditions. It is clear, therefore, that some construct is needed which has powerful but temporary inhibitory effects on performance. On the other hand, there is less as-

surance manifest on the particular characteristics which are assigned I_R . One of the problems concerns the "locus" of I_R . While the theoretician is not necessarily bound to locate anatomically a hypothetical process, Hull (40, p. 281) has suggested an effector locus. Ammons (4) concludes from his study of pursuit rotor learning that it is more probably a central, or at least a non-peripheral phenomenon.

Motor learning theories have also used the concept of conditioned inhibition (sI_R^2). Directly dependent on I_R , sI_R is a habit which develops when I_R forces cessation of activity; cessation being a reinforcing state of affairs (relief from negative motivation) the habit of not responding will be eventually cued to the stimuli present when resting occurs. It is the reviewer's opinion that conclusions drawn about the magnitude and characteristics of sI_R are untenable because of the large number of assumptions which are involved, some of them distinctly tenuous. The basic method of inferring sI_R has been, in simple form, to give one distributed condition and one highly massed condition. After a certain period of work, a rest interval is introduced. If the reminiscence effect for the massed group over the rest interval does not attain the level of performance shown by the distributed group after the interval, the difference is ascribed to sI_R . The following points are raised as complications of this method. (a) A basic assumption is that sI_R builds up only with massed practice. Since I_R generates very rapidly, many conditions of distributed practice should produce I_R . The formal rests given in spaced practice should allow for dissipation of I_R thus reinforcing the resting habit. For example, Duncan (18) found large reminiscence gains following distributed practice consisting of alternate periods of 10 sec. work and 20 sec. rest. Since reminiscence is used to infer I_R , its partial dissipation during the formal rest intervals should reinforce a habit of not responding. To say that I_R must reach some critical amount before sI_R is produced does not satisfactorily handle the problem since the critical level cannot be specified independently. (b) It has been assumed that the massed group learns as much as the distributed group even though the performance is poorer, i.e., even though fewer correct responses are made. That fewer correct responses are made under massing is especially clear in such tasks as reversed alphabet printing and block turning where discrete responses occur. Wasserman (96) recognized this problem and adjusted his values so that the number of responses made were approximately equal for massed and distributed groups. The consequence of this adjustment was to reduce the amount of decrement inferred to be produced by sI_R , but by no means did it eliminate the difference between massed and distributed groups on the initial trials following a reminiscence rest period. (c) Motor tasks may be influenced by warm-up, but as Ammons (3) has shown, warm-up decrement on the reminiscence trial is less following distributed than following massed practice. Therefore, some of the decrement attributed to sI_R following massing may be loss resulting from lack of warm-up. If practice after rest takes place by massing (as is usually the case), the peak performance which usually comes on the second

or third post-rest trial cannot be taken is an index of maximum learning for by this point I_R is again influencing performance. (d) The method used for determining gI_R assumes that there is neither forgetting of gI_R nor forgetting of the task being learned. This has never been demonstrated, uncomplicated by other factors for tasks commonly used in motor learning. (e) At least one study [Kimble (49)] has shown that reminiscence gains following massing will attain the level of performance of a distributed group. When considered as a whole, the method for inferring the process of gI_R is so top heavy with tenuous assumptions that the use of the concept is questionable. It is, therefore, disappointing to find that Hull's (41, pp. 76-77) primary experimental evidence for gI_R comes from the work in human motor learning.

EXTINCTION

Theories of extinction and of spontaneous recovery should be closely integrated. Yet, the facts of the case are that of the scores of studies which have taken extinction measures, very few have gone ahead to gather data on spontaneous recovery. The effect of this is that evaluation of theories of extinction is markedly hindered by the lack of knowledge of the parameters of spontaneous recovery. Until experimenters make a systematic practice of taking measures of spontaneous recovery following extinction, the confusion surrounding the evaluation of theories of extinction will remain. Deese (16) reports results which have considerable importance for theories of extinction. In a simple U-maze, animals were taught a spatial discrimination. Prior to extinction half of the animals were placed directly in the empty goal box for four 1-min. periods (nonresponse extinction). Following this, they were given typical extinction trials as was the control group not having nonresponse extinction. As shown by the regular extinction trials, nonresponse extinction had markedly reduced the response strength. Furthermore, the regular extinction trials were not followed by spontaneous recovery as were the trials for the group having only regular extinction. In short, a great deal of extinction can take place without the animal making the complete response to be extinguished. Theories which use inhibitory potential or reactive inhibition as the exclusive explanatory mechanism for extinction will have to be modified. The fact that spontaneous recovery (which may be attributed to reactive inhibition) occurs with regular extinction and not with nonresponse extinction may indicate that generation of reactive inhibition is not necessary for extinction but normally accompanies it. On the other hand, the Deese setup was such that considerable generalization within the maze would take place. The alley and goal box on one side were both black and on the other, white. Thus, a rat, being placed in the goal box, might have anticipatory goal responses extinguished, and then when placed in the maze for a regular extinction run would likewise have no anticipatory eating responses when the white (or black) stimulus arm is perceived prior to the choice. This experiment needs repeating with gradual variations in the similarities of the goal box and arms. Nevertheless, if it is true, as this study

indicates, that the reactive inhibition produced by running, inhibition which has been postulated to cause extinction, is no more than an epiphenomenon of extinction, an important contribution has been made.

Liberman (54) has stressed the importance of considering extinction and spontaneous recovery together when formulating theories. For Liberman, extinction consists essentially of learning not to respond [negative learning (53)]. Since there is no differential reinforcement during extinction, the tendency not to respond is associated with a large number of stimuli (as compared with positive learning where differential reinforcement is present). With the passage of time many of these stimuli will have other responses attached to them thus interfering with extinction. Therefore, since the negative learning is much weaker because of fewer supporting stimuli, spontaneous recovery of the original CR will take place. This expectation was supported (53) since if extinction took place by differential reinforcement procedures spontaneous recovery was less than under usual extinction procedures. Most recently Liberman (54) has made a direct test of a proposition from the theory, namely, that extinction—negative learning—transfers more broadly than does positive learning. One group of animals was trained on a runway followed by training in a Skinner box, with extinction in the same order. Another group had the same two tasks, but in the reverse order. Neither acquisition of the runway habit nor acquisition of the lever-pressing habit was influenced by having the other precede it. But extinction was considerably different, depending upon whether or not it was preceded by the extinction of the other task. If preceded by the other task, extinction was more rapid than if not so preceded. This supports the hypothesis that extinction transfers more broadly than does acquisition.

As far as theory is concerned, the above two studies are clearly not conclusive, but it would appear that we can no longer speak of simple inhibition theories versus simple interference theories. The extinction and recovery processes are more complex than these theories are capable of handling.

ROTE LEARNING PHENOMENA

Some form of a differential forgetting theory has long been used to account for the faster rate of learning of material with spaced as compared with massed practice. An interfering process is said to weaken more rapidly with rest than does the correct associative connections, thus leaving the latter relatively stronger when spaced practice is used. Any theory [e.g., McGeoch (59)] which identifies or measures the interfering processes with overt errors must be revised, since both Tsao (91) and Underwood (93, 94) have found that in serial learning more erroneous responses are made under distributed conditions than under massed. It is only fair to say, however, that in the Underwood studies the greater error frequency probably resulted from the activity used to fill the rest intervals and not from the interval per se. This will be demonstrated in a study not yet published. But, even with

this artifact removed no greater error frequency is found with massed than with distributed practice.

Only one study on reminiscence in verbal learning has appeared during the past year. This study, by Hovland & Kurtz (39), is notable because of their failure to find reminiscence under conditions nearly identical to those obtaining in experiments in which reminiscence was found in 1938 (38). Reminiscence can nearly be categorized as a prewar phenomenon. This inability to reproduce results over a period of years is a serious and perplexing problem. It is not only true for reminiscence but for other phenomena, among them, warm-up effects before recall in rote learning. In 1948, Irion (42) reported a remarkable discovery. If Ss were given 1 min. of warm-up before recalling a list of paired-associates learned 24 hr. previously, there was no forgetting. The warm-up consisted only of naming colors. The implications of this finding for forgetting theory are enormous. During the current year Irion & Wham (43) have shown the recall warm-up phenomenon using serial lists, although the results were not so striking as in Irion's original study. However, these results are not reproducible. Rockway [see McGeoch & Irion (60, pp. 453-55)] in a series of conditions was unable to produce any effect of a warm-up task at recall even when the conditions according to an evaluation by Irion (60) were exactly like Irion's original work. The only obvious difference to which one may appeal in such cases, or in the case of reminiscence cited above, is population difference. But just how these populations can differ so capriciously, and in something so fundamental (we presume) as the learning process, cannot be specified. It must be emphasized that the work referred to above concerns warm-up given just prior to recall. The results on giving warm-up before learning have been consistent. The most recent study [Thune (89)] used the learning of other lists as the warming-up activity. Actually, Ss learned 15 lists of paired adjectives, three lists a day for five days. The warm-up effect can be determined by comparing intra-day changes in performance with inter-day changes. Roughly, the third list learned on a given day required one-half the number of trials the first one did. But, the first list on a given day required several more trials than did the last list on the previous day. It is known from previous work [Hamilton (36)] that warm-up effects will not be shown if the interval between warm-up and learning is longer than 60 min. Learning-how-to-learn transfer, on the other hand, is relatively uninfluenced by the passage of time. Therefore, while some of Thune's intra-day changes are due to learning-how-to-learn, most of them can be ascribed to warm-up. One other study implies warm-up effects on learning. Worchel & Marks (99) discovered that if S slept $1\frac{1}{2}$ hr. just prior to learning a list of syllables his performance was inferior to that which followed $1\frac{1}{2}$ hr. of normal waking activity. Arising from a deep sleep to learn a list of nonsense syllables would be onerous enough in and of itself, but the interpretation of the poor performance may well be attributed to lack of warm-up.

TRANSFER

For the past few years occasional articles have appeared in popular magazines concerning learning during sleep. In no case, however, have reports been presented in standard research journals so that the procedure and results can be examined by students of learning. A report by Fox & Robbins (24), presenting positive results, now allows this critical appraisal so badly needed. It is included in this section on transfer because the insistence of the results rests in large part upon a condition producing interference. Three groups of 10 Ss were matched on ability to learn paired-associate lists, with Chinese words as stimuli and the English equivalents as responses. All conditions involved the playing of 30 min. of recorded material, the recordings being presented through a pillow speaker. The player automatically turned on at 2:30 A.M., and turned off at 3:00 A.M. For a control group waltzes were played for the 30 min.; for the first experimental group a list of 25 paired associates (Chinese and English equivalents correctly paired) was repeated 15 times. The second experimental group had the same list, but the Chinese and English words were paired at random. On the following morning all groups learned the list of pairs played during the night to the first experimental group. The results show that the first experimental group mastered the list most rapidly, being significantly faster than the control. The second experimental group, on the other hand, learned the list at a significantly slower rate than the control. Apparently, the learning during sleep, inappropriate for the list learned the following morning, interfered with the learning of the test list. In short, the behavior of Ss on the test list was exactly what would be predicted if the "sleep conditions" had been waking, knowing, learning conditions.

The history of science undoubtedly shows that many so-called new and startling discoveries turn out on close scrutiny to be "duds." We are, therefore, more prone to view such reported discoveries with considerable skepticism and distrust. In going over the Fox-Robbins study it is not easy to find any procedural difficulty which denies the reliability of the findings. Certain matters, however, may lead to questions. Of prime importance is that Ss were never actually observed asleep during the 30-min. period when the transcription played. However, 10 Ss were discarded who either said they heard the machine or awakened for some other reason. All 30 Ss on whom data are reported did not admit to awakening. If they are all liars, of course, the data show nothing. If part of them lied we have to have differential frequency in the groups in order to attain the reported results. It is also noted that the correlation between matching and test performance is about zero for all groups. But, the high homogeneity of the groups on the practice list would prohibit very high correlations. Such low variability among Ss on the test list is rarely found in normal transfer experiments with such material, but this may again only reflect the presence of a highly selected and homogeneous sample. In short, the reviewer has been unable to find critical errors.

If these results are confirmed, and certainly such a phenomenon needs verification before it becomes a part of the body of knowledge, some of us will need to revise our thinking concerning the roles of motivation and reinforcement in human learning.

Turning now to the more traditional studies of transfer, we shall consider a study by Rossman & Goss (76). In an earlier report by Gagné & Baker (25) it was found that connecting discriminating verbal responses to stimuli facilitated the acquisition of differential motor responses to the same stimuli. This has been interpreted in Gibson's terms (27) by saying that the verbal practice established differentiation (reduced generalization) among the stimuli so that S did not have to "go through" this process when learning the motor responses. Rossman & Goss essentially confirmed this finding. However, the importance of the study lies not in the quantitative data but in Ss' reports. On a formal questionnaire Ss reported that looking for and identifying parts of the fairly complex stimuli was the greatest source of facilitation produced by the verbal predifferentiation. Such a process may be no more than what is commonly called learning-how-to-learn. This suggests that in subsequent experiments it will be necessary that a control group be included in designs, this group having practice in discriminating stimuli different from those to be used in the motor task. It is conceivable that such a group could show as much transfer as would the experimental group which had practice on the stimuli actually used with the motor task. What may be learned is a technique by which generalization can be rapidly reduced, not the reduction of generalization *per se*, although more probably both factors will be shown to be operating. Clearly these factors must be measured independently in subsequent experiments.

The role of similarity in retroactive inhibition was studied by Bilodeau & Schlosberg (6), but the similarity was not contained in the material but in the environmental stimuli outside the list. In the critical condition, S learned the original list of paired adjectives in a light, airy room with the words being presented on cards. The interpolated list, on the other hand, was learned in a dingy black-walled storeroom, partially filled with old apparatus (a condition which any department could replicate), and S learned the words from a drum while standing. Thus, the context of the original and interpolated learning was distinctly different. It was reasoned that these context differences should reduce retroactive inhibition because different discriminative stimuli were present for original learning and interpolated learning. The expectation was clearly confirmed. While significant retroactive inhibition was produced in the condition with different contexts, it was significantly less than in a condition in which the context remained the same.

Retroactive inhibition in motor learning has been studied extensively by Lewis and his students. Only two (51, 57) of the studies are thus far available. The task used was the Mashburn apparatus, the interference being produced by complete reversal of light-movement relationships. The variables explored were degree of original and degree of interpolated learning.

As pointed out by these writers, while these variables have been extensively investigated in verbal learning, there is no *a priori* basis for assuming that the same relationships will be found for motor learning. The essential findings may be summarized: as degree of original learning increased, retroactive inhibition in relearning increased; as degree of interpolated learning increased, retroactive inhibition also increased. Although no consistent relationship was found between degree of original learning and transfer in learning the interpolated task, the analyses are clear in showing both positive and negative processes operating. For, not only was positive transfer evident in the number of correct responses made, but also evidence of interfering processes appeared in the intrusion of responses appropriate to the first task. The failure to find any consistent relationship between degree of original learning and transfer to interpolated learning is the only fact appreciable at variance with studies of verbal learning. Not apparent in these experiments, however, is the finding of verbal learning studies that maximal specific overt interference (when new responses are learned to old stimuli) occurs when two incompatible response tendencies are of about equal strength. That such occurs in animal learning has been neatly demonstrated by Rethlingshafer, Eshenbach & Stone (73). Animals were first trained to a high degree of proficiency to make a left turn for water (when thirsty) in a T-maze and a right turn when hungry; thus the basis for the discrimination was apparently differences in drive stimuli only. On test trials it was shown that if the animals were made both hungry and thirsty, maximal interference (50 per cent correct) occurred when the two drives had about the same strength as inferred from deprivation time. If there was a wide disparity in deprivation time for the two drives, interference was minimal. Cole (15) has also observed simple conflict behavior in monkeys when two opposed habits in discrimination learning were of about equal strength.

In general it would appear that current transfer of training studies utilizing verbal materials are now being performed to clarify theoretical problems, while those in motor learning are designed to secure much needed empirical laws.

SELECTIVE RETENTION

This term is used to cover a wide variety of operations but must not be interpreted necessarily to mean that there is a corresponding wide set of phenomena associated with the operations. At a descriptive level, selective retention studies have attempted to "personalize" experiences in various ways and compare the retention of these experiences with otherwise comparable but "de-personalized" experiences. The personalization attached usually reduces to some emotional component. If it were possible to prove the null hypothesis, a carefully recorded history of the work on selective forgetting would be a testament we would put first in the line of evidence. That research persists in this area in spite of so many previous negative results can probably be attributed to (a) the desire by many to bring the clinical

phenomenon of repression under scientific scrutiny, and (b) the new ideas for improvement in methodology to avoid criticisms made of previous work.

Some researchers in this area have studiously avoided the term "repression." Not so has Zeller (103) who made a frontal attack on the problem and designed a most methodologically precise study in an effort to demonstrate repression. This phenomenon, if demonstrated by his technique, would (according to Zeller) meet all the requirements of repression (after-ex-pulsion) as described by Freud. The criteria which Zeller outlined (102) for the demonstration are: (a) the material must be learned by S; (b) when an inhibitory factor is introduced recall must be reduced; and (c) when the inhibitory factor is removed the material is again remembered. The results of Zeller's experiment meet all three criteria, and while he is willing to conclude that the results are analogous to repression proper, he also realized that an alternative explanation in terms of reduced motivation is equally plausible.

A form of selective retention which may perhaps be independent of emotional factors is the well-known Zeigarnik effect. To demonstrate this effect a group of brief tasks is given S; some he is allowed to complete, and some he is not allowed to complete. If subsequent recall shows that the incompleting tasks are recalled better than the completed, the Zeigarnik effect is demonstrated. Boguslavsky (8) has analyzed this situation and has offered a series of postulates relating tension at the point of interruption and recall. Essentially these postulates hold that as a result of interruption the increased tension results in movement-produced stimuli which become cues for verbal responses related to recall. Equations representing certain relationships among the variables are presented. The most important implication of the theory is that under certain conditions recall of the completed task will be better than the recall of the uncompleted task—a reverse Zeigarnik effect. Unfortunately, the experimental evidence presented by Boguslavsky does not give much support to the formulation; even the Zeigarnik effect is not produced at a high level of statistical significance and the reverse effect is no more than a suggestive trend.

The Zeigarnik technique has been modified to introduce definite emotional components to the finished-task experiences and the unfinished-task experiences. Thus, if S is led to believe that whether or not the ability to finish tasks is evidence of intellectual ability (ego-involvement), recall of the finished and unfinished tasks may be different than in the relatively neutral Zeigarnik situation. Past experiments have been designed to include both the Zeigarnik situation and the ego-involved situation. The results of these experiments appear to demonstrate that under ego-involvement more of the completed tasks were remembered; again, a reverse Zeigarnik effect. Glixman (29) has previously criticized such studies largely on the grounds of inappropriate statistical comparisons, and Sears (77) was led to the conclusion that because of the complications produced by interruption there was little reason for continued use of the method as "a procedure for securing failure reactions in the study of repression or other memorial functions" (77, pp.

113-14). Alper (1) has come to the defense both of the technique and the statistical comparisons made. In the reviewer's opinion, several points concerning this series of studies are in order.

(a) Glixman's criticisms have been very valuable in forcing us to look carefully at the relationship between theory and particular statistical comparisons when the interruption technique is used. The effects produced by failure must be clearly separated from those produced by a relatively neutral situation.

(b) Sears's solution (discard the technique) is no solution. If diverse results have been found it probably means that certain methodological differences are present or that unidentified variables are operating. This state of affairs is quite normal in many fields of research in psychology and can only lead to more analytical research. The Zeigarnik effect need not confound the results when appropriate designs are used.

(c) Alper's emphasis on personality variables as the source of confusion ["future studies which use Ss unselected for personality structure will continue to yield diverse or equivocal findings" (p. 80)] is unfortunate. Probably most phenomena with which psychologists deal, from simple reaction time to complex social behavior, are determined in part by personality variables. But, this in no way prohibits the discovery of laws by the manipulation of environmental and task variables. Personality variables may be the cause of the diverse experimental findings, but this can only happen when the sample size and the design of the experiments have been inadequate, providing the populations from which the Ss have been drawn are comparable. If the Zeigarnik effect and the ego-involved effect are truly different, this can be demonstrated without reference to specific personality variables.

Finally, attention should be called to another matter which is partly a definitional problem and partly a methodological one. In the reviewer's opinion a distinction should be maintained between selective learning and selective retention (or selective recall). It may be one matter to account for differences in rate of acquisition of two associations and a different matter to account for differences in the retention of two associations originally learned to the same degree. If selective learning does take place as a function of motivational parameters, it clearly provides an explanatory problem. If selective retention takes place as a function of motivational factors for material originally learned to the same level, this too is an important finding requiring explanation. The explanations for such selective learning and selective retention may or may not be based on the same mechanisms. What should be avoided is the speaking of selective retention when the level of original learning of the material being retained is unknown. For example, in the reviewer's opinion little can be made of a recent study by Alper & Korchin (2) in which the authors speak of selective recall. Six men and 6 women were presented a passage in which pro-men and pro-women statements occurred along with anti-men and anti-women statements. Successive recalls were taken at various intervals starting at 10 min. following learning. On the

whole, differences on the first recall were maintained on successive recalls. Therefore, it seems more reasonable to refer to differences as due to selective learning rather than to selective recall since it is most likely that the differences reflect failure of fixation rather than selective weakening during the 10-min. interval between learning and first recall. In this case, however, differences between sexes may be due to differences in learning ability per se and not to selective fixation as a function of motivational differences associated with sex. True selective retention would be shown by convergence or divergence of the curves for the two sexes on the successive recalls. Such evidence is not presented. Differences were present between sexes on the first recall only for some types of material and not for others. This may represent selective learning, but should not be spoken of as selective retention. It should be emphasized again that adequate analysis may show that selective learning and selective retention (if found) may be handled by the same theory, but until that time at least, methodological clarity requires a distinction between the two.

THINKING

Research in this area continues to move at an appallingly slow pace. Nevertheless, there are rumblings which portend better things to come. The need for systematic research in this area is great; it arises from many quarters. Theory and laws accounting for simple conditioning have never been satisfactory for the clinician dealing with reaction formations, the peculiar thought processes of the schizophrenic, or the irrational perceptions of the paranoid. The armed forces demand help in understanding the processes by which a radar repairman does or does not find the "bug" in a disabled complex of wires, tubes, and resistors. And psychologists, finding considerable success for their theories at the less complex levels of behavior, are becoming more curious about how these theories will fit learning as observed in problem-solving situations, concept formation, etc. Such demands will sooner or later lead to standardization of techniques, calibration of materials, and consequent production of empirical laws with which theories will cope. Yet the rumblings are largely programmatic and the studies actually available for the past year are few in number.

Assuming that thinking involves the perception of relationships among stimuli, there is probably general agreement that variable behavior makes for most efficient thinking. If the stimuli involved are many, and if solution to the problem requires perception of a relationship among a very limited number of the stimuli, it is most apparent that variable behavior, on the basis of sheer probability, is going to lead to most rapid solution. However, the most concerted empirical approach to the role of variable behavior in thinking has been made by Guetzkow (31). Guetzkow's hypothesis is that there are three factors influencing variability. One of these, tending to reduce variability, is the traditional "set"—a term commonly used in one form or another as an "opposite" to variability. A second factor is that S may not have the broad

repertoire of responses available which can be evoked by a given situation. A third factor is the sheer lack of ability to form new solutions even though the elemental parts of solution are available. Guetzkow then proceeds to evaluate the usefulness of these conceptions. The results were conclusive only with respect to the set factor which will be the only topic considered here. Initially Ss were given the Luchin's-type (56) set problems. In these problems, S is given (hypothetically) three jars of differing but known size and is asked to obtain a given amount of water using only the three jars for measuring purposes. Five problems are given which can all be solved in the same way, i.e., by using all three jars and pouring from one to the other in the same sequence. These problems can be shown to establish a set to attack subsequent problems with the same formula. On test trials, S is given problems which can be solved by a short-cut method as well as by the method established in the first five problems. Finally, a problem is given which can only be solved by a simple method. The influence of the set is inferred from the performance on the test problems. Ss in Guetzkow's procedure were also given a paper and pencil test which is presumed to measure productive thinking, i.e., the ability to bring facts together into a new relationship. Guetzkow found that ability to get the first five water-jar problems correct in no way related to productive thinking as measured by the paper and pencil test. Furthermore, while men were significantly superior to women on the paper and pencil test, they did not differ on the solution of the first five water-jar problems. It is therefore clear that different abilities are being tapped by the two tasks. Guetzkow also used the Maier (61) two-string problem. In this problem two strings are dropped from the ceiling at such a distance apart that S cannot hold onto one and reach the other. His job is to tie the two strings together. The "best" solution is thought to be one in which a weight is tied on one string, set swinging, and then is caught by S while he retains the other. Those Ss who arrived at this solution scored significantly higher on the paper and pencil test than did those who did not. Thus, this solution to the string problem and the paper and pencil test are said to measure productive thinking while other solutions to the string problem (of which there are several) and the first five water-jar problems are said not to measure productive thinking. Further analysis of the water-jar problem allowed fairly clear-cut evidence for two distinct aspects of set, namely, the susceptibility to its induction and the ability to surmount it. Men and women were about equal in their susceptibility to the set established by the first five problems (and measured on the test problems), but men could surmount the set more rapidly than women. The same conclusion was evident in the data on the string problem.

Probably the most distinctive study on problem solving is that of Whitfield (97). In this study, problems were set up in which a precise measure of theoretical difficulty was available. Not only was the estimate of difficulty available at the start of solution but at any successive stage in solution. For

example, suppose S was asked to match four objects with four spaces on a board but there was no apparent relationship between the spaces and the objects. Since there are 24 possible ways in which the squares and spaces could be matched, the chance probabilities of being correct on the first trial are 1 in 24. But, on the first try S may get one correct. On the next trial, therefore, if he remembered which one was correct, the problem is reduced to getting three objects properly placed, which he might be expected to do by chance once in six times. The problems actually used were somewhat more complex. In one case Ss matched eight stimuli to eight spaces; in another, two objects to each of four spaces; and in another, four stimuli to each of two spaces. On the first trial, of course, S could only guess, but after his guess he was given certain information as to correctness (the specificity of this knowledge varied with each of the three problems). Then, all objects were removed and he was given another trial. Whitfield found that in the problem where eight objects and eight spaces were to be matched, performance was remarkably close to theoretically calculated performance from trial to trial. On the other two problems, however, performance lagged considerably behind predicted performance. The lack of correspondence in the latter two problems is attributed by Whitfield to the differences in the nature of the information given S on these problems as compared with the first. The greater the stress placed on memorial functions, the less the observed solution time matched the theoretical time. In short, the understanding of the memory functions may be an important key to the understanding of problem solving. This emphasis on memory, plus the straightforward application of probability thinking to the study of thinking are the major contributions in Whitfield's paper.

GENERAL

The trend toward mathematical models of the learning process continues. As illustrations of this trend, Bush & Mosteller have two papers, one dealing with instrumental conditioning (12) and one with stimulus generalization (13). These models have several purposes. In the first place, they are conceptual tools around which one can organize his thinking. Secondly, they provide precise ways by which one can describe relationships. Thirdly, they are presumed to generate predictions which can be tested experimentally. This latter is the critical point for ultimately assessing the worth of the model. Bush & Mosteller show that some available data can be fitted into the models, but the real value of these models, as Spence (83) points out, depends not on their capacity to absorb the available data (for the model was probably built *ad hoc* on these data), but on the unambiguously testable new propositions which are generated. Subsequent reviewers will have to evaluate how well the models will meet this test. One matter is clear; if the trend toward mathematical models continues to expand, as seems inevitable, the graduate curricula of many schools will have to be revised to allow for considerably more training in higher mathematics. Otherwise these theoretical

programs will not provide the stimulation needed for survival simply because they will lack readers.

General theoretical problems of learning have been discussed in a symposium published as separate papers by Mowrer (68), Birch & Bitterman (7), Sheffield (79), Kendler (47), Miller (63), and Ritchie (74). These papers are in general a restatement and summation of previously published points of view. They are good reading; they point up specific problems (e.g., interpretation of avoidance conditioning) on which theorists differ but, more than that, they are concerned with the central problem of what postulated processes shall the theorist use as the base of his system.

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VISION¹

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During the past year, a trend in the study of vision and visual perception which appeared in earlier years has become more marked. This is the tendency for investigators to fall into three main groups, differing greatly from one another in outlook and approach. The first of these groups adopts an analytical approach. Its members attempt to define, investigate, and measure separately, with great accuracy of detail, all the stimulus variables that appear relevant to the situation, and especially the physical and physiological variables. This approach is particularly well demonstrated in the chapters by Judd, Graham, and Bartley in the new *Handbook of Experimental Psychology* (132). Judd (132, pp. 811-67) considers mainly the physical parameters, Bartley (132, pp. 921-84) the physiological parameters, and Graham (132, pp. 868-920) the psychophysical parameters, of the visual and response situations. These are in all cases considered analytically, and the authors (especially Graham) claim that our knowledge is as yet insufficient to add up the effects of these various factors in such a way as to produce any general theory of the nature of our perceptions, or indeed to present much possibility of describing the phenomenal nature of a single perceptual act. Much data of this kind is also provided of course in the work of physicists and physiologists.²

In the second group of investigators are those, heirs perhaps to the Gestalt tradition (although they might repudiate this relationship) whose approach is predominantly configurational. An example of this approach was given by Gibson's book, *The Perception of the Visual World* (43). Gibson was particularly concerned to stress the integration of isolated and momentary impressions of the visual field as a basis of our understanding of the "visual world" as a whole. Helson (56) also emphasized the ordering of our experiences in a total pattern. But he advocated an accurate quantitative analysis of all the stimulus variables upon which these experiences are based, and has drawn up a list of the measurable perceptual dimensions and their main physical determinants. He advocated a further study of these, emphasizing

¹ The survey of the literature to which this review pertains was concluded in March, 1952.

² An important book, *The Sensations, their Functions, Processes and Mechanisms*, has just been published by Piéron (114a), which can only be briefly noted here. It covers the work of Piéron and his collaborators at the Sorbonne and the Collège de France, and other relevant work; and relates the visual to other sensory processes. Especially interesting is the discussion of the temporal aspects—the response time, growth, establishment, adaptation and decline of sensation. But other quantitative aspects—intensity and area of stimulation—are also included; and in addition, chromatic sensation.

however the importance of field conditions as a whole, as well as of the conditions of local stimulation.

The third group of investigators has adopted a very different approach to the study of visual perception. Its novelty has been proclaimed by terming it the "New Look," though in fact it originated in the work of F. C. Bartlett and in Boring's "context" theory. Thus it is opposed both to the analytic and the Gestalt approach, and emphasizes the importance of the hypotheses constructed by the individual about the nature and significance of the perceptual situation, and the extent to which these hypotheses are determined by experience, interest, and need satisfaction. The work of this group has been demonstrated particularly in *Perception: An Approach to Personality*, edited by Blake & Ramsey (16), and is expounded in various papers discussed below. It might be claimed that this work belongs to the field of personality study, or of social psychology. But it appears to the writer to give us more information about perceptual processes than about the nature of the personality or of social forces. It certainly needs assessment from the perceptual aspect, and some attempt at integration with the analytic and configurational data, as Helson (56) has clearly indicated.

We shall now proceed to consider in more detail the recent work on various visual phenomena. But it should be remembered that the results obtained are liable to reflect the predilections of one or other of the above groups. In particular, it should be noted that individual differences in results rarely appear in analytic studies, sometimes because only two observers were employed.

SENSORY PROCESSES IN VISUAL PERCEPTION

Visual acuity.—A useful review of the literature by Sloan (129) related measurements of visual acuity to illumination, degree of contrast, type and distance of test object, differences of visual angle subtended, and gradation of scale. Cruendet (31) pointed out that the photoreceptor response to light stimulation is proportional to the logarithm of stimulus intensity. Thus it appears, from this and from other data, that it would be better to use a logarithmic rather than a linear scale for measuring functions such as acuity and dark adaptation. In averaging measurements of these functions, geometric rather than arithmetic means should be calculated. Hofstetter (60) discussed the effects of various conditions on the measurement of visual acuity in eye testing, and made recommendations as to the optimum level and distribution of illumination.

O'Brien (105) has gone extensively into the histological basis of acuity, discussing the structure and distribution of cones in the fovea. He carried out experiments which seemed to show that the Stiles-Crawford effect is due to the tapering of cones to a narrow point, directed outwards, where the photo-sensitive pigment is located. He has also discussed, and dismissed as untenable, the theories of Hecht and Pirenne as to the causes of the relationship between acuity and luminance.

Ogle's experiments (107) showed that the ability to resolve two point sources of light, each subtending an angle of less than $10'$ of arc, depended on the brightness contrast between the points and the background, irrespective of the absolute brightness over a considerable range. When contrast was reduced below a certain value, of the order of 100 to 1, the degree of resolution became small and approximately constant. But Wilcox (148) found a different effect when the test object consisted of bright bars on a dark ground; resolution was at first improved but later impaired as the intensity of the bars increased. With small light figures on a dark ground, resolution became worse as intensity increased.

There have been several studies of individual variation in acuity. Parnell (111) found that visual acuity was poor in about 22 per cent of a group of 474 Oxford undergraduates, as compared with 5 per cent of men aged 18 in the general population of Britain. There was also a small deterioration of acuity among the undergraduates while they were at the University. A nontechnical book, *Eyes in Industry*, by Campbell, Riddell & Salusbury (28), has discussed at length the effect of industrial conditions on eyesight, and how they might be improved. The treatment of defects and diseases of the eye as they affect industrial workers was also discussed.

Bruce & Low (21) found that the acuity, as measured by the Landolt ring, of 118 naval flying cadets showed a significant improvement in central vision after eight weeks' practice in the identification of aircraft in brief and long exposures. Peripheral visual acuity, at 60° and 90° , did not improve. Thus it does not seem that the result could be attributed to factors of attention or quickness of response. A control group of medical students who did not receive training in aircraft recognition showed no improvement at a retest eight weeks after the original test.

A possible result of individual differences in visual acuity was discussed by Olmsted & Olmsted (108). They suggested that the differences found to occur between the descriptions given by different physiographers of distant hill and cliff formations might be attributed to limitations of their visual acuity. Olmsted & Olmsted estimated "normal" visual acuity in terms of the Snellen chart as $5'$ of angle, and stated that on this reckoning a cliff 100 ft. high would be only just perceptible as a discontinuity of outline at a distance of 13 miles. Hirsch & Weymouth (58) commented that, provided the cliffs were long enough, vernier discrimination would operate, with an acuity of $10''$, and a cliff 3.3 ft. high might be visible. But Olmsted & Olmsted (109) argued that factors such as atmospheric haze would prevent any discrimination comparable to that which occurs under laboratory conditions.

Brightness.—Judd, writing in the *Handbook of Experimental Psychology* (132, pp. 811–67), has given a useful account of the radiometric and photometric terms and scales, their meaning and use. A glossary of visual terminology was included at the end of his article.

Ratoosh & Graham (121) measured the effect upon the differential threshold for brightness of varying the size of the test field in relation to the size

of the surrounding adapting field. Two observers viewed monocularly fields varying from 0.17° to 1.34° of angle; the test field was exposed for .02 sec. They found that with the size of the test field constant, the threshold decreased as the size of the adapting field increased. With constant adapting field size, the threshold fell as the test field size increased from a very small size, remained constant at medium test field sizes, and rose as the test field size approached that of the adapting field, especially at higher adapting field brightnesses. They deduced that the sensitivity to brightness discrimination was increased by some process of interaction with a large adjacent illuminated field.

Bartley (9) has studied the apparent brightness of flickering fields and found that in certain cases this is greater than the apparent brightness of steady areas of equal illumination intensity. This "brightness enhancement" of the flickering field reaches a maximum at a rate of alternation of about 10 per sec. which approximates to the frequency of the alpha rhythm. Bartley suggested the hypothesis that some kind of driving effect takes place in the neural pathways to reinforce the alpha rhythm in the cortex. Clearly an intermittent stimulus of low intensity would not be expected to produce much effect. He found that a flickering ring, at high intensities, equalled the apparent brightness of a steadily illuminated disc which it surrounded, when the intensity of the former was only half that of the latter. But at low intensities the flickering ring had to be 14 times as intense as the disc to equal it in apparent brightness. In a further study (10), Bartley found that in general with weaker intensities of stimulation, a greater effect on apparent brightness was produced when a slower rate of flicker was used. This might be due to the fact that at weak intensities only a fraction of the maximally available number of neural elements was activated at each pulse. It might also be expected that a larger area of stimulation creating more stray light in the retina would produce more effect than a smaller area would. But this was not found to be the case. Bartley & Wilkinson (11) postulated further that a smaller brightness enhancement from the intermittent stimulation would occur if the total amount of stray light was kept constant. This was done by alternating the bright and dark phases of two adjacent areas of stimulation, and it was found that less enhancement occurred than with a single area of flickering light. Thus these results upheld Bartley's hypothesis.

Rudolph (128) found that after long sessions of matching brief light flashes of variable duration but constant brightness against test objects of constant duration, there were sporadic increases in the difference threshold for brightness. This effect was attributed to central fatigue rather than to peripheral effects, although the observer was unaware of any fatigue.

Flicker.—Considerable study has been made in the past year of individual differences in critical flicker frequency (cff), and of the conditions leading to such differences. Misiak (97) measured the cff for 182 normal males and females ranging in age from 7 to 89 years. Although there were large individual variations, there was a general tendency for the cff to fall in older ob-

servers. Misiak considered that the fall might be due to decrease in the size and mobility of the pupil. McNemar (93) found that the magnitude of individual differences in cff varied at different intensities, and also when different instruments, the strobotac and the episcope, were used. The differences in cff between individuals, and even the individual variability of response, varied from day to day. Knox (84) found that if some observers were instructed to report "flicker" as soon as flicker appeared, and others "fusion" as soon as fusion appeared, the former showed an increase of cff of about 35 per cent over a 10-day period of practice; while the latter showed a decrease of cff of about 12 per cent. The first group adopted an attitude of "looking for flicker," the second of "looking for fusion." Observers given neutral instructions showed approximate constancy of cff.

Battersby (12) and Battersby, Bender & Teuber (13) have published papers on the effect of cortical injury on the cff. It appeared that frontal lobe injury did not affect the cff when it was measured some time after the injury occurred—from four to six years after. But in occipital lobe injuries the cff was lowered, not only in the locus of the major defect, but also in the unaffected parts of the field. Battersby (12) considered that the structures between the lateral geniculate body and the area striata are of paramount importance in determining fusion.

Battersby, Bender & Teuber (13) also found that the linear increase of the foveal cff with the logarithm of the target area was due to increase of total light flux. When the latter was held constant, the cff was higher the smaller the area—that is to say, when more light energy was concentrated on the fovea. Winchell & Simonson (150) found that the cff increased with increase of the ratio of the light phase to the dark phase. But their experiments do not seem to have been well controlled.

Dark adaptation.—Wald (140) has discussed the results of his experiments on the breakdown of rhodopsin by light to retinene and opsin, and the further reduction of retinene to vitamin A₁, and their resynthesis in darkness. He has further described the construction of an artificial system in which the bleaching of rhodopsin results in electrical variations, which may be a model for the excitation processes in rod vision.

Mote & Riopelle (99) investigated foveal dark adaptation in relation to varying intensities (11300, 5650, 1130, and 565 m μ L.) and varying durations (300, 150, 30, and 15 sec.) of preadaptation period. A Hecht-Schlaer monocular adaptometer was used. As either intensity or duration of the pre-exposure increased, the initial foveal threshold rose, the slope of the dark adaptation curve decreased, and the time to reach the final threshold, which was in the neighbourhood of 7.0 log m μ L. for both subjects of the experiment, was increased; but this time was never more than 5–7 min. The dark adaptation curves for the two higher intensities and longer durations were the same whenever $I \times t = C$. For the two lower intensities and shorter durations they were also the same, but were different from those for the higher intensities and longer durations.

Long (89) found that for flashes of varying intensity and duration, after 30 min. of dark adaptation, $I \times t = C$ in peripheral vision up to a duration of 0.10 sec. After that, there was an abrupt transition to $I = C$. Variation in the wave form of the flash (that is to say, sudden or gradual onset and decline) did not vary the effect. Wolf & Zigler (151) and Zigler, Wolf & King (154) measured the effects of length of exposure of test patch, and of brightness of surround, on dark adaptation. The longer the exposure of the test patch, over a range of 0.1 to 1.0 sec., the lower the threshold; this lowering was relatively greater in the parafovea, at 6° from the centre, than in the fovea. Increasing the brightness of the surround over an area of 40° from 0 to 40 m μ L raised the threshold. Between 0.4 and 4.0 m μ L, the rod section of the dark adaptation curve disappeared altogether, and the level of the cone section was raised. Pre-exposure to ultraviolet light also raised the rod threshold.

Riopelle & Hake (125) measured the monocular absolute thresholds for momentarily exposed segments of annular stimuli, the segments varying in area over a range of 32 to 1, at angles of 7.5° and 20° in directions temporal and inferior to the fovea. The relationship between area and intensity was much the same whatever the position of stimulation, although the inferior area of the retina appeared to be less sensitive than the temporal area. But the relationship was not completely reciprocal. An increase of area in the ratio 1 to 32 was equivalent to an increase of intensity in the ratio 1 to 3. The two subjects of the experiment showed significant differences of absolute threshold, but the same general trend of results. Riopelle (124) varied the form of the stimulus, using a varying number of circles of different sizes stimulating an area 20° from the fovea. The threshold was higher when the dispersion of the circles was greater; and also when the size of the circles was smaller and their number greater. It was highest with 64 circles each $1/8$ in. in diameter. Thus summation is greater, and the stimulus units interact most, when they are nearest together. Denton & Pirenne (34) projected flashes of green light ($.518 \mu\mu$) of 0.0026 sec. duration on two areas, each subtending $10'$ of arc, at different distances apart in the peripheral retina of the dark adapted eye. A certain degree of summation, but not complete, occurred when the areas were 0.5° apart; but none occurred when they were 3.5° apart.

Hulburt (66) studied the effects of preadaptation to six different coloured lights, equal in brightness to a white field of 6 foot-candles. He also compared the effects of red and white preadaptation fields of varying brightnesses. In all cases dark adaptation, measured by the time required to recognize a black spot on a screen illuminated at 8 m μ L, was quicker after preadaptation to the longer wavelengths than to the shorter wave lengths. It was also quicker for red than for white preadaptation fields, especially at higher brightnesses. Preadaptation to ultraviolet light of low intensities caused little disturbance of dark adaptation. This result seems to conflict with that noted by Zigler, Wolf & King (154).

Johnson & Riggs (76) superimposed absolute threshold curves obtained during dark adaptation upon the B-wave electroretinal response curves of the same observers. The curves were roughly comparable, but did not show exact correspondence; the electroretinal response curves did not flatten out, but continued to fall during prolonged dark adaptation. They concluded that, although the scotopic visual system is responsible for both phenomena, yet there must be changes in physiological organization during dark adaptation which cannot be readily related to concentration of rhodopsin. Boynton & Riggs (17) measured combinations of area and intensity necessary to produce B-waves of various magnitudes in the electroretinogram, after 45-min. dark adaptation. Over a range of area from 0.11 to 10.2 mm.², the effect produced (on magnitude and latency of the B-wave) was the same for any area provided the same ratio of area to intensity was maintained. Thus

$$\log I = -k \log A + C,$$

k being unity at the fovea and 0.86 to 0.93 in the periphery. The responses from the blind spot were similar in magnitude to those from the rest of the periphery; those from the fovea were only slightly smaller. Boynton & Riggs attributed these effects to the scotopic mechanism, activated by the total amount of stray light within the eye. Thus the effect was much the same in all parts of the retina.

Results obtained by Krieger & Bender (85) also seemed to indicate alterations in physiological organization. They found that dark adaptation was affected by lesions in the optic pathways although the retina was normal. In an earlier paper (13a) they had shown that such patients, found to be scotomatous by routine perimetry, nevertheless were able to perceive and localize faintly luminous targets in darkness. Dark adaptation took place, but there was a general rise of threshold and increased variability of response; adaptation proceeded at a slower rate than the normal; and the usual point of inflection was lacking between the curves of cone and rod adaptation. But the fact that dark adaptation was abnormal in such individuals might indicate a general interference with visual function, and not necessarily any interruption of adaptation at the retinal level.

Motokawa, Ebe, Arakawa & Oikawa (101) investigated the effect of exposure to flashes of light of low intensity in the peripheral retina on the excitability to momentary electrical stimulation, given at a period 4.5 sec. after exposure. They showed that all cone response had disappeared by this time, and that only rod responses were occurring. The curve of degree of excitation at 4.5 sec. after exposure to various wavelengths coincided closely with Hecht's scotopic visibility curve. The lower the intensity of the preilluminating light, the further the displacement towards the periphery of the region of maximal response. It seems probable, therefore, that the more peripheral rods have a higher sensitivity.

Karwoski & Wayner (79) investigated the curious phenomena which occur when a bright line of light is moved rapidly across the field of the dark

adapted eye. An afterimage appears which is sometimes brighter than the original sensation; it may bulge in the middle as the line crosses the fovea, and this bulge may appear "gouged out" at the centre. These phenomena they attributed to the fact that afterimages in the cone area are weaker in the dark adapted eye than those in the rod area. But the former have a shorter latency and tend temporally to inhibit the latter which appear as delayed responses.

The relation of "practical night vision" to dark adaptation was studied by Eckel (36). Various instrumental tests of dark adaptation were made; and night vision was measured by the ability to perceive moving objects through a telescope at night. There was some correlation between this measure and the dark adaptation thresholds at the fifth and fifteenth minutes; but none with the final dark adaptation threshold. However, those with very good night vision showed relatively rapid dark adaptation. Discrepancies between night vision and dark adaptation thresholds were attributed to the effects of psychological factors such as concentration and judgment.

Glare.—Various studies have been made of sensitivity to glare and the conditions in which glare affects different people. Guth (51, 52) found that the amount of distraction produced by glaring lights varied with the size, brightness, position, and number of light sources. He determined what degree of each of these factors produced a just noticeable sensation of discomfort; and obtained equations relating their effects to each other. For instance, when the brightness of a source of light was increased, it was necessary to increase field brightness, or to decrease the size of the source, to maintain comfort. He calculated the effects of linear, as well as of circular, light sources.

In determining the effect of lighting and its distribution in, for instance, a factory, Lay (86a) suggested that it is desirable to divide up the field of view into four zones: the "task" (a central area 1° in diameter); the central field (30° in diameter); the surround (an annular zone around the central field extending to 90° diameter); and the peripheral field outside this. None of these zones should be brighter than that within it. "Task" brightness should be maximal; but efficiency within this zone would be affected if the brightness of the central zone much exceeded one-fifth that of the "task" zone. Other conditions of glare, and the effects of contrast within the various zones, were discussed. These points seem valuable as suggestions, but require experimental verification. Glatt (46) reported that production managers in factories have noted a drop in industrial efficiency after changing from incandescent to fluorescent lighting.

Hopkinson's data (62) showed that the sensitivity to discomfort caused by glare varies with previous experience in the observers. The experienced observer is quicker to note abnormal or unpleasant conditions than the less experienced. Peterson & Simonson (112) found that glare which had no effect on the accommodation of young observers (students), produced a definite lengthening of the near distance of accommodation in older people. They

attributed this effect to the increased absorption of heat-producing rays in the older lenses.

Colour vision.—A long and detailed exposition has been given by Judd in the *Handbook of Experimental Psychology* (132, pp. 811–67) of Grassman's laws and three colour equations for the normal observer, with reduced classifications for cases of colour defect. Colour perception and its dimensions were also discussed; its analysis in terms of the Maxwell triangle, and its various modifications; and the various phenomena of colour adaptation, including transformation or constancy. Judd pointed out that the analytical method has so far been unable to produce data which can be resynthesized to predict what colour perception will occur in duplex fields. It is not yet clear if this is due to errors in evaluation of the component factors, or whether some essential aspect has been overlooked. It is of course clear what would be the configurationists' answer to this question.

Wright (153), in the seventeenth Thomas Young oration to the Physical Society of Great Britain, discussed briefly the development of colorimetry and photometry, and some of their modern problems; also, the developments in colorimetric and photometric instruments.

A very extensive review of the processes and mechanisms of colour vision has been presented in two papers by Talbot (134, 135). In the first (134), he noted the necessity of postulating the coupling of the blue and yellow receptor functions, and of a black mechanism linked with white; of the linking of the blue receptor mechanism with rhodopsin processes, as in colour adaptation, colour vision in the periphery, in small areas of excitation and with momentary stimulation; and of the independence of yellow from red and green. In the second paper (135), he discussed the physiological basis of these processes—the photosensitive pigments, and the synaptic mechanisms which can produce from three absorption processes, four colour primaries at the bipolar level, and six selectivities at the fibre level. This discussion is far too complex to reproduce here, but it is clearly of great importance to a knowledge of the theory of colour vision.

Hartridge's book, *Recent Advances in the Physiology of Vision* (54) naturally gave considerable attention to his polychromatic theory. But it also gave a full description of experimental work on colour vision—colour matching, hue discrimination, retinal adaptation, peripheral colour vision, and colour blindness.

Hartridge has claimed more recently (55) that the retinal pigments isolated by Dartnall (32) showed absorption bands the wavelengths of which corresponded to the wavelengths of maximal retinal sensitivity as determined by Hartridge's fixation method, and also to the wavelengths of Granit's "modulators." Hartridge also found that the crests of the retinal pigment absorption bands were separated by equal frequency intervals. Collins & Morton (30), however, disputed the existence of some of these pigments, and stated that the absorption band wavelengths of others did not

correspond accurately with Hartridge's values for retinal sensitivity and Granit's "modulator" values.

In further experimental work on hue discrimination Thomson & Trezona (136) used a field of $1^{\circ} 20'$ in a Wright colorimeter. With reduction of luminance, discrimination deteriorated in the same proportion for all wavelengths between 620 and 490 $m\mu$. But this effect did not appear to be the same as that claimed by Hartridge to be produced by reducing the size of the field, since the latter was tritanopic, with greater deterioration in the yellow than in the blue green. Thomson & Trezona also found no fourth minimum in the hue discrimination curve in the range 610 to 640 $m\mu$. and attributed that found by previous experimenters to a faulty method of presenting results. Brown (19) obtained a tritanopic effect in the fovea when field luminance dropped below about 1 ft. L. He tested the colour discrimination of two observers making colour matches on a colorimeter with a two-part circular field subtending an angle of 2° , and a dark surrounding field. A similar effect was obtained by Middleton & Mayo (94). Their three observers were required to name the colours of 2° squares with very dark surrounds. At greatly reduced illuminations, the colours became more blue; green appeared as blue, red and orange as purple. They pointed out the importance of these effects in the discrimination of colour signals at low intensities of illumination.

Weale (145) found no tritanopic effect for a test object subtending $50'$ in the fovea. But at angles of 10° and 15° , there was considerable deterioration in hue discrimination in the region 480 to 510 $m\mu$, amounting to tritanopia at low brightnesses. There was also a deterioration in hue discrimination at low brightnesses at 460 and 590 $m\mu$ at 10° and 15° , suggesting that there might be dichromacy of the deuteranopic type in the peripheral retina.

Hsia & Graham (65) made extensive investigations of the colour sensitivity of the dark adapted fovea to a $42'$ field, exposed for 4 msec. They confirmed earlier findings that there was a distinct depression of sensitivity at 460 $m\mu$, and a rather less depression at 600 $m\mu$. The two alterations in direction of the sensitivity curves seemed to indicate the existence of three colour components. Weale also (146), obtaining luminosity curves to a field of $50'$ at 0° , 10° , and 15° , found regions of enhanced sensitivity to red, green, and blue in the fovea and parafovea, and additional sensitivity to yellow in the parafovea. However, he pointed out that the latter might be caused by a fusion of red and green responses, and not necessarily by a special yellow response mechanism. Motokawa, Ebe, Arakawa & Oikawa (102) carried out further experiments on the colour sensitivity of the foveal and peripheral retina, using Motokawa's method of measuring the sensitivity to momentary electrical stimulation at varying intervals of time after 2-sec. exposures to $2'$ test patches of wavelengths 610, 575, 520, and 460 $m\mu$. After red and green stimulation the sensitivity of the fovea was greater than after blue and yellow stimulation; in the latter case, response was sometimes completely lacking. This then seems to confirm the tritanopic effect with small areas of stimula-

tion in the fovea. In the parafovea, the sensitivity after blue and yellow stimulation was greater than for the fovea, but the sensitivity after red and green stimulation was less. At 20° the response after yellow stimulation was greatly increased, and that after green reduced. Yellow was also most conspicuously effective in the response curves of dichromats and anomalous trichromats. Ebe, Isobe & Motokawa (35) pointed out that in normals the electrical response curves were humped after exposure to red, green, and blue in the fovea, and to yellow also in the parafovea. But in protanopes and deuteranopes, responses after red, blue, or green stimulation might be abnormally weak, with response to yellow occurring strongly in the fovea as well as in the parafovea. The differences of response between dichromats and anomalous trichromats were merely quantitative, and not qualitative.

In another investigation using the measurement of electrical excitability after exposure to light of different colours, Motokawa (100) studied induction effects. He found that the response was "neutralized" if a complementarily coloured stimulus was interposed between the original inducing colour and the electrical excitation. Indirect induction effects of a complementary nature spread over the retina for some distance from the point of stimulation. These indirect effects could then be neutralized by interposing a stimulus of the same colour as the original inducing stimulus. The indirect effects did not spread across the blind spot, and were also blocked by prestimulation with white light, or by any device which broke up the homogeneity of the field. Koffka's well-known demonstration, in which a grey ring was superimposed half on a red and half on a green field, was investigated, and the effects could be shown to be due to indirect induction and neutralization.

In an experiment on colour adaptation, Hochberg, Triebel & Seaman (59) employed a technique in which both eyes were covered by translucent eye caps, the size of half tennis balls, which fitted closely over the eyes against the face. These were illuminated from without by monochromatic light, thus exposing the eyes to homogeneous, coloured fields. The field at first appeared like a coloured fog, but at length complete adaptation took place, and the field was seen as black or dark grey. This took different periods of time for different observers. When at this point the dark shadow of a rod was cast on the eye caps, it was seen as black surrounded by a halo of the original colour. Closing the eyes for 2 sec. produced the complementary colour, and reopening them a brief flash of the original colour. A purely retinal explanation of these phenomena, such as that the adapted receptors cease to respond, does not seem feasible; possibly the neural responses were inhibited.

Experiments were performed on the Hecht effect of the binocular fusion of red and green to produce yellow. Gunter (53) found that observers who had difficulty in making this fusion sometimes succeeded more easily with fields of 2° or less. It could also be obtained when the eyes were stimulated simultaneously or alternately with flickering red and green lights, with relatively small fields, and with low intensities. Furthermore, observers who

did not actually perceive fusion to yellow produced a conditioned psychogalvanic response to the mixture when they had previously been conditioned to yellow. This also occurred with red-green defectives. Hurvich & Jameson (67) attributed the Hecht effect to lack of purity of the red and green stimuli. They found that the only wavelengths in the spectrum yielding pure hue sensations were in the blue, green, and yellow; and these had narrow ranges which were different for different observers. Pure red could be produced only by a mixture of spectral 671 $m\mu$ and 440 $m\mu$. When this and pure green were used to stimulate the two eyes, the resultant sensation was neutral and colourless. Walls (143), however, has criticized these results, pointing out that other experimenters, using the same wavelengths as Hurvich & Jameson for discrete monocular stimulation, obtained fusion to yellow. Apparently Hurvich & Jameson also carried out this latter experiment and obtained a neutral white. Walls suggested that because they exposed the dark adapted fovea to bright light, the observers were dazzled by glare and saw white rather than yellow.

Burnham (25) found that the excitation purity of coloured patches increased as their area increased, especially between areas subtending 2° and 22° of visual angle. There was also a small shift towards the red end of the spectrum. Some colours became brighter in appearance, others darker. Hurvich & Jameson (68, 69) found that the temperature of a light source necessary to produce a pure white sensation varied with the luminance of the test object; at a higher luminance, an increased temperature range could be employed. But test stimuli of 6500°K. to 7500°K. were seen as white over a wider range of test conditions than were any other stimuli. The size of the test object was also important. The effect was rather difficult to determine, since it depended on the relative numbers of chromatic and achromatic receptors stimulated. The white sensations appeared most readily when the eye was preadapted to a test field at a colour temperature slightly below that of the adapting field. The full results cannot be presented here, but they suggested to Hurvich & Jameson that there must be a separate mechanism in the eye for response to white, which varies independently of the colour receiving mechanisms.

Further observations have been made on variations in colour vision and defective colour vision. Alexander (3) has stated that there are two types of total colour blindness: achromatopsia, in which there is also visual acuity of less than 6/60, photophobia, and nystagmus; and monochromatopsia, in which visual acuity is 6/60 or better, and there is no photophobia or nystagmus. Achromatopsia is agreed to be due to absence of functioning cones. Monochromatopsia he attributes either to some defect in the central pathways, for instance, a lack of the midget bipolar cells which occur in the cone but not in the rod pathways; or to the existence of atypical cones, for instance, cones sensitive to one wavelength only.

Kephart & Tieszen (81) have compared the scores of normals and colour defectives on the Ortho-Rater (a Kodachrome transparency slide of four

of the Ishihara plates) with their performance on the whole Ishihara test and the Pseudo-isochromatic plates. It was found that by slightly modifying the scoring of the Ortho-Rater it could be made to classify correctly in one of two groups tested 97 per cent of those found normal on the Ishihara and Pseudo-isochromatic tests, and 100 per cent of those found to be defective.

A pure albino woman aged between 45 and 50 who was photophobic, nystagmoid, and had a strong squint, was tested by Pickford (114) using his four-colour test and the Ishihara test. She was found to be red anomalous without darkening of the red, and also blue-yellow weak with three times the normal blue-yellow matching range. Jaeger (74) has instanced the case of a combination of two types of abnormal trichromatic vision in a woman who was deuteranomalous and to some extent tritanomalous. She had two sons, one a deuteranope and the other a protanope. Her sister was a protanope and had a son who was a deuteranope. Hitherto it has been thought that nonallelomorph compounds, such as these women, have normal colour vision. Jaeger assumed that the first woman combined three independent forms of colour defect, protanopia, deuteranomaly and tritanomaly, but manifested only deuteranomaly and tritanomaly. But her sister combined protanopia and deuteranomaly, and was a manifest protanope.

Binocular phenomena.—In an extensive investigation comparing the vision of coal miners with that of the normal population of Britain at various ages, Campbell, Harrison & Vertigen (26) found that stereoscopic acuity was poorer in cases of refractive error and decreased with age. It seems to be relatively unaffected by dark adaptation, and is often higher among coal miners who have worked many years underground than in other individuals of the same age. It breaks down in cases of nystagmus but can be improved by orthoptic training (27).

Howarth (63) found that whereas visual acuity (as measured by separation of two parallel lines) decreased with decreasing intensity of illumination, stereoscopic acuity increased slightly. This somewhat surprising result was attributed to the fact that at lower illuminations the pupil is dilated, and moving objects go more quickly out of focus, the blurring assisting the judgment of nonequidistance. Richards (122) compared stereoscopic acuity with vernier acuity. He measured the effect of rate of alternation between stereoscopic and pseudoscopic fields, and found that stereoscopic discrimination fell rapidly as the rate of alternation increased, becoming impossible above four alternations per sec. When right and left views of a nonstereoscopic field were alternated, vernier acuity also decreased but much less rapidly. Richards concluded that the central processes necessary in stereoscopic vision for integrating the two retinal images could not occur at speeds higher than those of four alternations per second.

Wilner, Weymouth & Hirsch (149) investigated the effect of changes in the brightness of the background on stereoscopic acuity as measured by the Howard-Dolman apparatus. Acuity was greater when there was a high degree of contrast between the rods of the apparatus and the background. If

the rods were unequally bright, there was a tendency to place the brighter further away. The authors attributed this effect to the increase of apparent size by irradiation; but it seems possible that it was due to a direct effect of the apparent nearness of relatively bright objects.

Trumbull (138) has tested the Verhoeff stereopter and found it to have a high test-retest reliability, of the order of .80. It has the advantage of eliminating all cues for distance other than that of binocular parallax. Various scoring methods can be adopted according to the accuracy of discrimination required.

Wright (152) studied stereoscopic acuity when the angle between the standard or reference object and a series of test objects at different distances was varied. The objects subtended angles of 25' of arc at the eye. All distance cues other than convergence, disparity, and accommodation were eliminated. Observations were made under two conditions: (a) when the observer (one only) looked backwards and forwards between the two objects; (b) when the reference object was fixated and the test object viewed extra-foveally. Stereoscopic acuity decreased as the angle of separation increased, more rapidly under the second than under the first condition. Wright considered that this was because convergence sensations became relatively more important than retinal disparity in determining distance judgments as the separation increased. It seems equally probable that in the second condition the extra-foveal percept became too blurred for accurate localization. Wright also considered that acuity at an angle of separation of 14° (which was about half that for neighbouring angles), under condition (a) must have been due to convergence alone, since here the field presented to one eye fell on the blind spot, and thus there can have been no disparity impressions. This result is difficult to interpret.

Leyzorek (88) found that the ability to judge the relative distances of two points exposed successively was good even with a 16-sec. time interval between exposures. Surprisingly enough, the judgment was as accurate when the points were viewed in darkness with only a single definite fixation point, as when they were seen in peripheral vision in daylight.

Riggs & Ratliff (123) have pointed out that the "corresponding points" of binocular vision are in fact based upon a temporal summation of the graded responses of a number of receptor cells, since fixation is never maintained accurately for more than a very short time. They obtained binocular records of eye movements, using reflecting contact lenses on each eye. Though large drifts and tremors were co-ordinated in the two eyes, small tremors (up to 15° rotation at a frequency of 90 per sec.) were quite independent. Thus small degrees of disparity between the two eyes must be based upon temporally integrated mean separations of varying instantaneous degrees of correspondence. Ogle (106) found that, when other conditions were properly controlled, stereopsis occurred outside Panum's "areas of fusion," though not beyond certain limits—that is to say, up to an angular separation of about 1° at the fovea, and 4° at an angle of 10° from the fovea.

The effect of factors other than disparity on depth perception was shown by Adamson (1). He exposed two drawings of horizontal arrows, one longer than the other, in a stereoscope. A tilted arrow was observed, and its angle of tilt could be matched against that of an actually tilted arrow. The observed tilt was less than the theoretical tilt, and was also less when fixation was maintained than when the observer was allowed to scan the field. It seems that the depth impressions depended not only on disparity but also on the extent to which the visual field could be observed. Again, Miles (95) has pointed out that the horopter is not a fixed surface, but is flexible, varying with movement to and fro of the fixated object, which causes changes of convergence, and also with heterophoria, aniseikonia, etc.

Cibis & Haber (29) have discussed the various factors, inherent and artificially produced, which can lead to anisopia—the unequal vision of the two eyes which produces unequal retinal images. This manifests itself in a characteristic distortion of space. For instance, in the "Venetian blind effect," if a neutral colour filter is placed before one eye, two targets in front of the eyes will appear rotated to one side. This effect can also be produced by putting an artificial pupil in front of one eye, or by a spherical or cylindrical lens causing blurring, or by bleaching one retina by exposing it to a bright source of light.

As regards the effects of binocular summation and inhibition, Misiak & Lozito (98) found that binocular afterimages have a longer duration and a shorter latency than have monocular afterimages. These effects they attributed to brightness enhancement of the binocular impressions, produced by interaction between the retinocerebral apparatuses of the two eyes. Asher (6) found that if in a stereoscope the right eye viewed a dimly lit test patch on a dark ground, and the left eye a similar test patch on a bright ground, the resulting impression was a bright background with two patches on it, the right bright and the left one darker than it—that is to say, black. The original brightness contrast remained the same for each eye, even when they were simultaneously stimulated. The impression from that part of the bright background in the left retinal area upon which was superimposed the patch from the right eye must have been inhibited.

Alexander (2) found that the rate of alternation in binocular rivalry was more rapid with "strong" figures with firm outlines and high contrast, than with "weak" figures with dotted outlines and low contrast.

In discussing the problems of ocular dominance, Walls (142) suggested that there is one type, motor in character, which expresses itself in a monocular sighting preference which is independent of the laterality of the brain that governs handedness. He postulated that the "visual ego" was located in this dominant eye, and sighted from it and chose it for monocular tasks. Motor impulses were sent to this eye alone, and the other eye tracked its movements in a reflex manner.

Accommodation and chromatic aberration.—Otero (110), discussing the relationship of accommodation to vision in dim light, stated that he has

demonstrated the existence of "night myopia." In darkness, the eyes are not accommodated at infinity but to a distance of 1 to 1.5 m. This accommodation must be altered for both near and far vision, and this apparently is impeded in dim light. Spherical and chromatic aberration may also increase the blurring of the image in dim light. Fincham (41) considered that, in general, accommodation is largely dependent on degree of convergence, rather than upon the blurring of the retinal image. Thus accommodation is more accurate when scanning movement takes place than when the eyes are fixated. But nevertheless accommodation may be inaccurate in monochromatic illumination when no clue is given by chromatic aberration. Both chromatic aberration and the Stiles-Crawford effect stimulate accommodation. Karwoski & Lloyd (78) also found that blurring produced by chromatic aberration affected accommodation, and hence localization of objects. Thus accuracy of localization of coloured lights decreased in the order yellow, red, green, and blue, as chromatic aberration decreased.

Lepicque (87), discussing the depth effects created by painters in their pictures, suggested that the differing chromatic aberration of different colours played a part in creating the impression of far distance.

CONFIGURATIONAL PHENOMENA IN VISUAL PERCEPTION

Figural aftereffects.—Bevan (14) measured the absolute threshold (by gradually increasing the illumination of the field) of dots located inside or outside simple geometrical inspection figures (circle, square, and triangle), fixated for 45 or 90 sec. The figural aftereffect resulted in a higher threshold for dots in positions falling inside that of the inspection figures than for those outside. When the inspection figure was a narrow vertical line of light 276 cm. long, and the test figure a dot at varying distances away, the threshold for appearance of the dot varied with its distance from the position of the line. Fox (42) obtained results which differed from Köhler's both as to the amount and as to the direction of displacement, when the inspection figures were nonsymmetrically placed, and the test figures symmetrical. Further experiments seemed to show that only part of the effect was due to satiation, and the remainder to "adaptation to the normal" as postulated by Gibson.

Transparency.—Prentice, Krinsky & Barker (119) have repeated the experiments of Fuchs, Tudor Hart, and Heider on transparency, but have not confirmed their results. When the transparency was produced by a blue episcotister rotating in a plane in front of a yellow background (of equal brightness), the latter showed no increased yellowness other than that produced by normal colour contrast. Nor was there any difference of yellowness when half the background was behind the transparency and half outside it. The authors concluded that the effects found by Fuchs, Tudor Hart, and Heider may have been due to the small number of observers used, and to a failure to equate the brightness of the blue and yellow surfaces.

Apparent movement.—Ammons & Weitz (5) studied the appearance of the phi phenomenon by alternating bright vertical lines, separated by 10°

of angle, (a) monocularly, (b) with one line to one eye and the other to the other; in some cases the stimuli were presented on either side of the fixation point, that is, to opposite hemispheres; and in others, with both stimuli on the same side of the fixation point, that is, to the same hemisphere. Phi movement was reported in all cases, but more clearly with monocular than with binocular stimulation. The movement appeared shorter when only one hemisphere was stimulated than when both were stimulated. The first observation supported Bartley's hypothesis that there is summation at sub-cortical neural levels; the second showed that some cortical summative effect may also occur.

Ekman³⁸ has described the slow apparent movement or "wandering phenomenon," discovered by Johanssen (75), which occurs when two points of light have their intensities increased and decreased alternately, so that when one is bright the other is dim. The light appears to swing from the position of one light to that of the other, as if attached to a pendulum. The temporal interval is much longer than that of the phi phenomenon. Ekman put forward a series of equations for the phenomenon and the conditions under which it occurs.

Constancy phenomena.—Ittelson (73) has published a general discussion of the nature and functions of constancy phenomena. He postulated that constancy behaviour shows an attempt to create and maintain a world which deviates as little as possible from the world of past experience, since this attempt offers the observer the best opportunity for effective and successful action. Thus, when stimulus conditions are ambiguous, the observer constructs an "assumptive" world, on the basis of his reactions towards it. Apparent distance is dependent upon what the object appears to be, as a "thing," the latter being determined by the assumptions made about the nature of the thing and its relations to the world in general. This theory seems to explain why we are able to identify objects successfully under different conditions of distance, orientation and illumination, but not the fact that our normal judgments of distance, size, shape, and colour depend upon the setting of the objects within the visual framework. It should be noted that the outlook of Ittelson and Ames, with its stress on "meaning," is closely associated with that of the "New Look" and opposed to that of Gestalt psychology.

Various studies have been made showing the dependence of shape and size judgments upon our knowledge of the nature of the objects perceived, as long as no perception of their relationship to the spatial framework is possible. Following up an earlier paper (71), in which he showed that the estimated distance of stationary objects, seen in monocular vision, depended upon what was thought to be their actual size, Ittelson (72) found a similar effect with moving objects. Objects, the sizes of which were varied steadily, appeared in monocular vision to move in exactly the same way as did really moving objects. When seen binocularly (but with surroundings concealed), a change of size still appeared as movement, though in some cases the objects

seemed to move "without getting anywhere." When a half-sized, or double-sized, playing card was moved to and fro, the distance of movement was estimated to be much less, or much greater, than that of a normal-sized playing card. Again, Ittelson (120, pp. 81-7) showed that if an object were made to move to and fro, while at the same time its size was altered so that it subtended a constant visual angle at the eye, it appeared to remain motionless. If the size was altered so that the visual angle subtended decreased as the object approached, it appeared to recede (and vice versa). The effect persisted to some extent in binocular vision. If the object was moved at right angles to the line of sight, the motion could be made to appear circular by suitably modifying the size of the object. A similar, more elaborate demonstration was made by Ames (4), though it should be noted that no experimental results were obtained. (It is somewhat difficult to describe this demonstration without showing the appropriate figures.) When a window of trapezoidal shape was rotated about a vertical axis, and viewed monocularly, or binocularly from a distance of 25 ft., it appeared as a rectangular window continually changing its size, and oscillating at a continually varying speed through a small sector of the complete circle of revolution. A cube fastened to it appeared to become detached at certain points and to fly through the air; while a tube passed through the frame seemed bent by the pressure on it of the mullions of the window. But in all cases, the operative factor was claimed to be expectation based on previous experience of the "most likely" interpretation of the perceived changes.

But in an experiment rather similar to that of Ames, Langdon (86) showed his observers a vertical, circular wire frame, covered with fluorescent paint, which was seen monocularly rotating at one of five speeds in a dark room. It had to be matched for shape against a graduated series of fifteen ellipses. Shape constancy increased from 0.098 to 0.225 as the speed of rotation increased. When the observer himself rotated the circle by hand, shape constancy was zero. It is of course comprehensible that constancy was much lower than in Ames's experiment, since there was less impression of a familiar solid object. But apparently the rotatory movement in itself was able to impart some impression of "reality," and hence give rise to some degree of constancy.

Smith (130) again tested the relationship between size and apparent movement. The observers viewed monocularly three types of drawing, some meaningless, some representational (one, for instance, a drawing of a baseball); and their size was increased or decreased steadily. The onset of apparent movement aroused by the change of size varied greatly between different observers who used different criteria of judgment. But the threshold for perception of movement was no lower for the drawing of the baseball than for the meaningless diagrams, which does not seem altogether surprising.

Gilinsky (45) derived two interrelated formulae for perceived size and distance as functions of true size and distance. Degree of constancy was related to the maximum limit of perceived distance for each observer under

the condition of observation. The formulae agreed well with the results of Holway & Boring (61), and also with the results obtained by Gilinsky herself from experiments on (a) judgment of equal appearing intervals of length at distances of 80 ft.; (b) bisection of distances varying from 8 to 200 ft.; (c) estimated size of the moon when seen just above the horizon.

Edwards & Boring (37a) have pointed out the relations of Emmert's Law to size constancy. Although Emmert's Law was ambiguously stated, it can be deduced from the experiments he described that he was really stating that the apparent size of the afterimage varied with the distance to which it was projected, and was assuming that complete size constancy was operating under the conditions of his experiment.

The relation of size constancy to the spatial framework has been found to vary with age in children. Piaget & Lambercier (113) also found a variation in judgments of perspective size (that is to say, retinal image size). These were most accurate at 7 to 8 years, decreased in accuracy from 8 to 10 years, and then improved again, though even adults did not do so well as the 8- to 10-year-olds. This was interpreted as showing that the youngest children were least influenced by size constancy, but that their percepts became more linked to the spatial framework as they grew older. Afterwards, however, they became more able to detach parts of the field from the framework and to make abstract judgments of perspective size.

Finally, Graham, writing in the *Handbook of Experimental Psychology* (132, pp. 868-920), has stressed the need for further accurate analytic data covering all the various factors and conditions in which size constancy operates—especially on the influence of intensity, duration, and area of stimuli, and of the angular separation of standard and variable. An earlier paper by Joynson (77) gave some interesting data on the last of these factors.

VISUAL PERCEPTION OF POSITION AND MOVEMENT

Extensive studies have been made of the effects produced when an observer's orientation to the visual field comes into conflict with his orientation in accordance with the postural senses. This is a matter of considerable importance in flying; and a symposium on the subject has been published by the United States Office of Naval Research (120). Graybiel (120, pp. 3-8) pointed out that when an aircraft is not flying in the horizontal position, the external visual cues given by the horizon, and the indirect visual cues given by instrument readings, conflict with the cues depending on the visual framework of the aircraft itself and also with the postural cues. The external visual cues may be weakened by poor visibility, and the gravitational forces strengthened by high speed spin. Thus there may be a strong tendency to rely on cues given by the latter rather than on the indirect instrumental cues which are not as compelling as direct cues.

Experiments were reported on the ability of observers to estimate the vertical position when visual cues were weak or absent and gravitational cues distorted. Graybiel & Brown (49) found that observers, rotated facing

the direction of movement in the human centrifuge, accurately adjusted a line to what appeared to them to be the vertical in accordance with the resultant of gravitational and rotational forces. There was a time lag of about 1 min. in making this setting after acceleration to a steady speed, and a similar lag when rotation was stopped. In this case the visual framework (other than that provided by the structure of the centrifuge) was almost eliminated. Mann (120, pp. 30-35) stated that when observers could perceive a competing visual field, their judgments of the vertical were a compromise between visual and gravitational cues. Witkin (120, pp. 118-29), summarizing his earlier experiments, pointed out that there were marked individual variations, some observers relying mainly on visual cues, others on gravitational plus rotational cues. Furthermore, he claimed to have shown that the latter, who were able to liberate themselves to a greater degree from a disorientated visual framework, appeared, from their performance on the Rorschach test, Thematic Apperception Test, etc., to have more self-assurance, activity, self-awareness, body confidence, absence of anxiety, etc. These differences began to appear as early as eight years of age. There was a general decrease in reliance on the visual framework between 10 and 17 years, with some tendency to return to it subsequently, especially among women. Since the airman is required, according to Graybiel, to liberate himself from the gravitational rather than from the visual framework, these results seem a little unfortunate if they are to be accepted. But possibly the self-assured individual can liberate himself from any distorted and unreliable framework. Finally, Gibson (120, pp. 77-80) pointed out that the apparent contradiction between the results of Graybiel and Mann, and those of Witkin, might be due to the fact that when people are confronted with an ambiguous perceptual situation, with conflicting cues, their perceptions are unstable and may be determined by individual attitudes, expectations, etc. Thus the factor of expectation might be very important in the flying situation.

Werner, Wapner & Chandler (147) found that the apparent vertical was displaced merely by tilting the observer's body while he was viewing a luminous vertical rod in a dark room. A more marked displacement of the vertical was demonstrated by Wapner, Werner & Morant (144) in observers sitting in a spinning chair. Though women seemed to show rather less displacement than men showed, individual differences of the kind found by Witkin (120, pp. 118-29) were not demonstrated. These results were held to support Werner's theory of "sensori-tonic" postural adjustment.

Walls (141) has prepared an extensive historical review of the literature relating to the problem of visual direction, that is to say, of the upright appearance of the field although the retinal images are inverted.

Proprioceptive factors appeared to be operating in target aiming experiments carried out by Szafran (133). He required people of various ages to locate targets with a pointer, in accordance with a series of signals which indicated which target was to be touched. The older observers deteriorated much more in their performance than did the younger ones when they were

required to wear goggles which concealed the targets and left only the signals visible. It appeared that the younger observers were more able to use proprioceptive data in locating the hidden targets.

Other effects of rotation have been studied. Thus, Imus, Graybiel, Brown & Niven (70) found that if the observer sat facing the centre of rotation, with his head fixed, the visual fixation point appeared to rise as rotation accelerated, and the observer felt as if he were being tilted backwards in his chair. Wendt (120, pp. 36-41) showed that cessation of acceleration, or of movement, produced, after the primary nystagmus set up by the original movement, a secondary nystagmus in the opposite direction. Fixating a strong, stationary field decreased the secondary nystagmus. Sometimes if the individual was in a dreamy inward-looking state, this nystagmus was succeeded by wandering, nonconjugate movements of the eyes, as in sleep. Mann, Guedry & Ray (91) called them "postural negative aftereffects." They were prevented by focussing the attention. Clark & Graybiel (120, pp. 55-9) found that there was a lag in the onset of the nystagmoid movements such that, if the period of rotation was short, the nystagmoid effect produced by acceleration would be more or less cancelled out by the effect produced by deceleration and cessation of movement. Thus a short spin of an aircraft might set up no dangerous nystagmus.

Prolonged secondary nystagmus resulted, however, in the perception of apparent movement of the visual surroundings when fixated; this is called the oculo-gyral illusion. Mann, Guedry & Ray (91) found that it occurred even when rotation had taken place in darkness; thus it was due to postural and not to visual factors. Guedry (120, pp. 67-9) found that it lasted longer in older than in younger observers. But Brown & Guedry (18) showed that the illusion was reduced by habituation, if the observer viewed a target while rotating, or if the room light was turned on for 5 sec. at an interval of 2 sec. after the cessation of rotation. Grossfield (50) has pointed out that the perceived movement of objects can be distinguished from movement of the observer's own body only when a system of reference always at rest relative to the observer's body is placed in the external world. Thus it is to be expected that the clearer the external visual field, the more quickly is the oculo-gyral illusion destroyed.

A further type of illusory movement has also been found to constitute a flying hazard, namely, autokinetic movement. This occurs frequently in night flying, and may lead to aircraft accidents. Imus, Graybiel, Brown & Niven (70) found that it could be controlled by improving the lighting of the aircraft, and by the pilot's shifting his gaze. Edwards & Crutchfield (37) found that the extent and duration of autokinetic movement was reduced under laboratory conditions when a point with a semicircle to one side of it was fixated first. If fixation lasted for 60 sec. only, the greatest reduction of autokinetic movement occurred when the semicircle was placed in the same direction from the fixation point as the prevailing direction of autokinetic movement. But a longer fixation (140 sec.) reduced the autokinetic movement

whatever the direction of the semicircle. All these results point to the necessity of strengthening the visual framework if disorientating eye movements are to be eliminated.

EFFECT ON VISUAL PERCEPTION OF OBJECTIVE FIELD CONDITIONS

Numerousness.—Minturn & Reese (96) have distinguished two types of estimation of number in short periods of viewing. One operates with numbers of objects up to six, the other with larger numbers. Practice in estimation with knowledge of results increased accuracy, particularly in cutting out extreme estimates. The improvement occurred quickly and soon reached a maximum, but persisted to some extent after an eight-month interval. But the tendency to use different methods of estimation for small and large numbers was not affected by practice. Volkmann, Reese & Corbin (139) showed that the first type of estimation was quick and accurate; the second, quick but inaccurate and lacking in confidence.

Reading.—Visual acuity as measured by the ability to distinguish between the letters T, R, L, and B on an optician's chart was found by Kephart & Pecsok (80) to correlate highly with the ability to make this distinction when the charts were exposed tachistoscopically for 1/5 sec. Thus they concluded that the normal acuity tests give a satisfactory measure of rapid perception.

Dearborn, Johnston & Carmichael (33) claimed that the comprehensibility of typewritten material could be improved by various changes in format which emphasized the meaningful structure of the sentences; for instance, typing the most important word in each sentence in capital letters; breaking up the material into sections forming units of thought; blackening important sections by retyping over the original copy; setting up the material in two columns of short single-spaced lines. Rather similar changes were recommended by North & Jenkins (104) for printed material. They found that students read magazine articles more quickly when the latter were printed with extra spaces between phrases or between small meaningfully connected groups of words; it was said that this procedure emphasized the meaningful units of the material.

Howes & Solomon (64) found that the time taken to read words exposed tachistoscopically correlated about $-.70$ with their frequency in the Thorndike-Lorge word count, but was also affected by the legibility of letters and the pattern of letters within the word. Postman, Bruner & Walk (116) demonstrated once more the well-known effect, that a misprint—in this case a reversed consonant—is much harder to detect when it is included in a meaningful word than in a series of consonants. The reversed letters sometimes slowed down the perception of the other consonants, but not of the meaningful words.

Brozek & Simonson (20) investigated the effect of illumination on the fatigue caused by reading letters which passed a slit at irregular intervals or irregular levels. After 2 hr., there was scarcely any decrement in rate of

reading with an illumination of 100 foot-candles; but an increasingly greater decrement as the illumination decreased to 2 foot-candles, and some decrement when it was increased to 300 foot-candles. The blink rate was affected in the same way. The effects were less, however, when a slightly greenish source of light was used.

Tinker (137), however, found that rate of reading newspaper type for short periods was constant above an illumination of 7 foot-candles, and for book type above 3.1 foot-candles. He has criticized the use of Weston's formula, based on the relation of visual acuity for the Landolt ring to brightness contrast, because it would indicate a very much higher optimal illumination for tasks such as reading. Threshold data cannot necessarily be applied to suprathreshold tasks. It seems quite probable also that the effect of illumination on reading rate involves other than sensory data. Moreover, the results of Brozek & Simonson would suggest a high optimal illumination when long periods of reading are involved.

Scale reading and tracking.—An excellent discussion of earlier work on this subject has been given by Fitts (132, pp. 1287–1340). Barber & Garner (8) required observers to read off points on three types of scale, graduated in 20's, 10's, and 5's respectively for equal intervals. The observers had to estimate their readings to the nearest one-twentieth of a marked graduation. There was a strong tendency to estimate in whole numbers, and to do so either to the nearest marked graduation or to a point midway between marks. Thus readings were most accurate on the scale graduated in 20's.

Graham, Baxter, & Browne (48) compared the use of vertical, horizontal, and circular scales in target tracking. The observers were required to keep a pointer adjusted at a fixed point by rotating a control knob placed below the scale, while the pointer was being moved irregularly. Both the tracking errors and the number of times the knob was turned in the wrong direction were least with the horizontal scale; and on the whole they were greater with the vertical than with the circular scale. In a further study, Graham (47) investigated the relative accuracy of tracking in the four quadrants of the circular scale. Accuracy was as great in the top quadrant as on the horizontal scale, but was less for the two lateral and the bottom quadrants. In particular, there was a tendency to turn the control knob in the wrong direction when, as with the bottom quadrant, the relationship between turning and direction of movement of the tracking pointer was reversed.

INDIVIDUAL DIFFERENCES IN VISUAL PERCEPTION

We have already mentioned the book edited by Blake & Ramsey (16) which contained a number of papers on various aspects of the influence of personality factors, social attitudes, and individual needs on perception. There has been considerable discussion of these subjects in the *Psychological Review*. Luchins (90) accused the "New Look" psychologists of overemphasizing group trends and subjective wishes, whereas the Gestalt psychologists would have regarded them as only a part of the total system of stresses

determining the individual's perceptions. Bruner (22) countered by saying that it is in ambiguous situations, where the objective factors are deficient, that personality factors lead to the formation of hypotheses about the perceptual situation. But Gibson (44) has pointed out that artificial ambiguity has frequently been created in perceptual experiments by the use of outline forms rather than of real objects or surfaces. The processes involved in perceiving the former are different from those operating with the latter, since outline forms "stand for" real objects, or symbolize geometrical conventions, in a fluid and unstable way. Murphy & Hochberg (103) have formulated 18 hypotheses which cover some of the Gestalt principles, the development of differentiation and integration in children, the schematic interrelation of exteroceptive and proprioceptive data (which we have discussed above), the function of need in determining "set" to perceive, especially when sensory data are reduced, and the irreversibility of the final integration when it provides a permanently satisfying anchorage. This seems to be a valuable attempt to reconcile and integrate the different approaches to the study of perception outlined at the beginning of this review.

Klein & Schlesinger (82) investigated the ability of two groups of observers to see apparent movement. One group was characterized by close adherence to objective form in the Rorschach ink-blot test, lack of imaginative freedom, and restricted and stereotyped responses; the other, by greater lability and freedom of imagination. The first group was able to see apparent movement over a significantly greater range of speeds of alternation of various configurations than was the second. Eriksen (40) found that observers who were relatively quick to perceive the content of tachistoscopically exposed pictures of aggressive scenes, produced more Thematic Apperception Test stories with aggressive themes than did observers who were relatively slow to perceive such pictures. In another experiment, Eriksen (39) found a lag in the perception of pictures of scenes relating to aggression and homosexuality, among certain psychiatric patients, but not in those who showed overt aggressiveness and homosexuality in their behaviour. This lag was correlated with signs of disturbance in the word association test, in response to words related to these two needs. There was also a tendency to distort or neglect the main features of the pictures before they were fully recognized. Erikson considered that, although a certain degree of general emotional disturbance was operating, yet there was also some "perceptual defense" set up by repression of the need for aggression or homosexuality.

Some of the earlier work on individual differences in tachistoscopic perception of words may be criticized on the grounds that the differences of familiarity of the words were not taken into account. Solomon & Howes (131) repeated the experiment of Postman, Bruner & McGinnies (115) on the perception of words associated with specific values (from the Allport-Vernon Study of Values test). Groups of familiar and unfamiliar words (based on frequency in the Thorndike-Lorge word count) were selected for each value. There was a considerable and significant difference of reading

time between familiar and unfamiliar words, but very little between those of greatest and least value. But the latter difference was somewhat increased in observers who showed extreme differences on the Allport-Vernon scale. Postman & Schneider (118) repeated the experiment in much the same form, and found a close relationship between reading time and value rank for relatively unfamiliar words, but not for the more familiar. It thus appears that the individual may choose to become familiar with rather unusual words within his own range of interest, but not outside it. At a subsequent recall of the words presented, value rank was of much more importance than familiarity in determining which words were remembered. Further, Postman & Leytham (117) found that the names of desirable personality traits were not more quickly perceived than the names of undesirable personality traits (the familiarity factor being controlled). Names of traits were, however, more quickly perceived when the observers thought that other people would agree with them as to the degree to which they themselves possessed these traits, than when they thought there would be disagreement.

Rosenstock (126) carried out a modification of another earlier experiment by Bruner & Postman (24), but again without checking the factor of familiarity of the words used. Rosenstock presented sentences the second clauses of which had highly sexual or aggressive connotations, while others were neutral. He used different degrees of illumination, and found that a greater intensity of illumination was required to perceive the emotionally toned sentences. Women observers were slower to see those dealing with aggression, and men observers were slower to see those with sexual references; and the errors introduced showed the same trends. As in the experiment by Bruner & Postman, these effects were attributed to repression. McGinnies & Sherman (92) found that the inhibitory effect of taboo (sexual) words could spread and retard the recognition of neutral words (controlled for degree of familiarity) exposed immediately after them. They attributed this effect to the creation of a state of anxiety and embarrassment by the taboo words, which set up a "perceptual defense" or avoidance of subsequent perception.

Rosenthal (127) checked the experiment of Bruner & Goodman (23) on the estimated size of coins of different denominations. In Rosenthal's experiment, children had to adjust the size of a circle of light on a screen to equal that of various coins of ascending order of monetary value. When estimation was made from memory of the size of the coins, the poorer children overestimated the size of the more valuable coins to a greater degree than did the richer children. When they held the coin in the hand, the poorer children showed a gradual drop in overestimation as their ages increased from 6 to 10 years; thus their overestimation was greater than that of the richer children at 6 years, but less at 10 years. Ashley & Runyon (7) found that hypnotized observers tended to underestimate the size of coins when it had been suggested to them under hypnosis that they had "rich" life histories, and overestimate them when it had been suggested that they had "poor" life histories. The difference existed even when they were shown actual coins, but it

was greater when they had to estimate from memory. Klein, Schlesinger & Meister (83) found that no effect was produced on size estimation of discs by value as indicated by printing a dollar sign or a swastika on the discs, even with German refugee observers. But a few observers showed persistent tendencies to under- or overestimate sizes, whatever the circumstances. Bevan & Dukes (15), however, claimed that accuracy of relative distance judgments was greater when the distances of two 20-dollar bills were compared than when two neutral stimuli were used (with the visual framework concealed). This effect was not obtained when one-dollar bills or replicas of bills were used. Thus it could not be attributed to the detailed patterning of the 20-dollar bills, but was said to be due to the greater alertness of the observers when viewing them.

Hinckley & Rethlingshafer (57) found that short men tended to overestimate heights of other men, whereas tall men tended to underestimate them, within a medium range of 5 ft. 9 in. to 6 ft. 1 in. But these tendencies did not appear to the same extent with heights outside this range. Thus the results seem to have been due to the "central tendency of judgment" which operates in psychophysical experiments, rather than to a social preference for men of medium height.

These observations mark one pole of the scale of perceptual judgments, the opposite pole being constituted by the accurate and disinterested judgments made, for instance, in experiments on the absolute threshold and visual acuity. There is a gradual transition from one pole to the other, as more, and more varied, subjective and interpretative factors come into operation. But we are sometimes inclined to forget that even accurate psychophysical judgment depends on the attitudes and meanings acquired during training to perform such experiments.

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HEARING^{1,2}

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Over 500 publications on hearing appeared in the last year. It would take almost exactly the space of this chapter to list them. That might be the best way to use the space if it were not for the fact that the Psycho-Acoustic Laboratory is bringing up to date the *Bibliography in Audition* (63). Since that work will fulfill the bibliographic function, this chapter will confine itself to a few major topics on which particularly significant work has been done in the last year: (a) the action of the cochlea, (b) detection and discrimination, (c) subjective attributes of sound, and (d) the hearing of speech and complex, speechlike sounds.

THE COCHLEA

In a sense, the study of the action of the cochlea is not psychology. The experimental work is done mainly by physicists and electrophysiologists, and the theorists are principally physicists and communications engineers. Yet the important problems in the psychology of hearing reduce, in large part, to questions about the action of the cochlea. A few recent papers have done much to consolidate our ideas about the mechanical analysis made by the cochlea and, thereby, to lay a groundwork for study of further stages of the process of hearing. Consequently, this chapter starts with, and considers at some length, recent developments in the field of that small organ.

Mathematical theory of the action of the cochlea.—Békésy's³ observations on the physical properties and the mechanical action of the cochlea provided: (a) a picture of a mechanism sufficiently clear to suggest a differential equation, (b) measurements of the independent variables precise enough to permit numerical computation, and (c) measurements of the dependent variables sufficiently definitive to check a theoretical solution. Several theorists have taken advantage of this opportunity. The most recent developments constitute perhaps the culmination of one phase, the exploitation of a particular differential equation.

The equation (a hydrodynamical form of the telegrapher's equation

¹ The survey of the literature to which this review pertains was completed in May, 1952.

² The preparation of this chapter was supported in part by a contract between the Air Force Cambridge Research Center and Massachusetts Institute of Technology.

³ Békésy's observations were described first in European journals. In 1947, two of the most important papers were republished, in English, in the *Journal of the Acoustical Society of America*. Now, essentially the whole story is available in English in Chapter 27 of the new *Handbook of Experimental Psychology* (11).

that describes the propagation of a signal along a line consisting of distributed impedances) is the basis of the more-or-less independently constructed theories of Ranke (74, 75), Zwislöcki (91, 92), Peterson & Bogert (66), and Fletcher (33). Since Ranke has considerably revised his earlier treatment (72, 73), it is probably fair to consider Zwislöcki's 1948 paper the first of the current theories. Zwislöcki gave a complete formulation of the action of the cochlea, but he assumed that the length of the wave that travels down the cochlea is long relative to the transverse dimensions of the cochlea, which is the case up to about 4000 c.p.s., and he simplified the computation by neglecting mass, which he regarded as playing a minor role. Then Peterson & Bogert (66) set up the problem somewhat differently, retaining the assumption of long wave length but neglecting friction instead of mass. And then Ranke's (74) revised theory appeared, giving a treatment of short waves as well as long ones and, if my understanding is correct, taking into account all three forms of impedance, but in a somewhat devious way.

From one point of view, the contribution of recent months has been the solution of the problem of how to take all three forms of impedance into account simultaneously and directly in the application of the fundamental dynamic equation. First, Bogert (18) obtained a solution by building an electrical analogue of the hydrodynamic model. The analogue was a cascade of 175 sections, each consisting of inductors, capacitors, and resistors chosen to mimic the values of mass, stiffness, and viscosity measured by Békésy. The behavior of the model in response to voltages impressed upon its "stapes" was strikingly similar to that of the human ear in Békésy's observations, and it differed in the expected way from the behavior of the frictionless (i.e., resistanceless) model of Peterson & Bogert. Then Fletcher (33) worked through the computations numerically, with mass, stiffness, and friction all in the picture. His solutions are in excellent agreement with curves shown by Békésy for the velocity of the traveling wave, the positions of maximum response, etc.

From another point of view, however, it appears that the contribution of recent months is to show how different can be rival formulations that start from the same physical measurements, employ the same fundamental equation, and check well with the same empirical observations.⁴ Although

⁴ When it comes to checking against Békésy's observations, the situation is not as clear as I have implied. Actually, Békésy presented two sets of curves for one of the most basic functions, the one relating displacement amplitude to position along the cochlear partition. [Originally, they appeared in different papers, but they can be seen in Figures 32 and 35 of reference (11).] The two sets of curves are very different in a most important respect: the spread of response along the length of the cochlea is about three times as great in one as in the other. Phase curves are presented only with the narrow displacement-amplitude curves. A complete set of frequency-selectivity curves is presented only with the broad displacement-amplitude curves. Zwislöcki chose the frequency-selectivity curves (broad) against which to check his theoretical calculations, whereas Fletcher's calculations yield curves that approximate Békésy's

there are many features in common among Zwislocki's, Peterson & Bogert's, Ranke's, and Fletcher's formulations, there are important differences. These lie in associating aspects of the actual situation with variables in the model. As Fletcher puts it,

Although the fundamental differential equation which is used is the same as that used by Peterson and Bogert and Zwislocki, except for a term involving the viscosity of the fluid, the interpretation of the constants and the boundary conditions are very different. (33, p. 637.)

In so far as there is—despite the differences of interpretation—consensus on general features, the current theory of the action of the cochlea (as I understand it) holds that: The acoustic (stimulus) pressure wave acts upon the normal ear principally through the drum skin and ossicular chain, which it sets into mechanical vibration. The vibration of the footplate of the stapes moves the fluid of the cochlea. The movements of the fluid are compression-rarefaction waves (I shall call them simply compression waves), but the fluid is so nearly incompressible that the vibration is propagated at extremely high velocity (approximately the velocity of sound in water) and therefore reaches all parts of the cochlea almost in the same phase. The compression waves set up vibration in a system consisting of the cochlear partition and fluid on either side of it. The coupling between the compression waves in the fluid and the partition-fluid system occurs principally, or almost entirely, in the base of the cochlea where the partition is stiff. In this basal part of the cochlea, all the segments (into which the partition is, for convenience, conceptually divided) vibrate together in essentially the same phase. From this part, the vibrations travel up the partition-fluid system, which acts as a transmission line. Energy flows from one segment to the next, principally through the fluid and not to any great extent through the partition itself.

The passage of the traveling wave along the partition-fluid system is very different from the passage of the compression wave through the fluid. The traveling wave is governed by the differential equation that serves as the common basis for the current theories. The frequency-selective action of the cochlea is due to the nonuniform distribution of impedances along the partition-fluid transmission line. The response of the cochlea to any stimulus is calculated by determining, for that stimulus, the difference in pressure (as a function of time) between the fluid near the oval window and fluid near the round window, and using that difference as a boundary condition for the differential equation. At the same time, of course, it is necessary to "plug in" the values of the impedances from Békésy's measurements, ter-

narrow displacement-amplitude curves. When Huggins and I were working on the problem of sharpening mechanisms (44), we asked Békésy which of his two sets of curves was the more representative. His recollection was that the set with broad maxima was some kind of an average, whereas the set with sharp peaks was from a single, and somewhat unusual, preparation. However, Békésy had continually improved his technique, and the single preparation was one of his last ones in Europe.

minating the line with an impedance characteristic of the helicotrema.

Transmission-line theory versus parallel-resonator theory.—Against the interpretation just described, it is necessary to set an argument developed by Wever & Lawrence (89). Their picture of the action of the cochlea is a modern version of the classical resonance theory associated with Helmholtz. They start with the compression wave that passes at high velocity through the fluid of the cochlea. Instead of influencing primarily the stiff part, i.e., the basal turn, this compression wave acts directly upon all the segments of the cochlear partition. The coupling of these segments, one to another, is negligible, and each responds in a manner conditioned by its own resonance (its local impedance) and by the pressure difference produced across it, between the neighboring fluids in the vestibular and tympanic canals, by the compression wave. The essential feature is that there are, conceptually, many parallel resonators, all driven almost simultaneously (because of its high velocity of propagation) by the compression wave. This is basically different from the situation postulated by the traveling wave theories because, according to them, the compression wave acts chiefly on one part (the basal end) of the cochlear partition, and the principal component of the action of the rest of the partition is due to the passage of the traveling wave down the partition-fluid system, which if it is viewed as consisting of resonators, consists of many elements in cascade and not in parallel.

The evidence considered critical against the traveling wave theories by Wever & Lawrence is the following: They introduced sinusoidal vibrations into both ends of the cochlea and adjusted the amplitudes and phases until there was complete cancellation in the microphonic recorded from a particular location. Then, moving the active electrode, they found that there was complete or near complete cancellation also at all other locations. This could not be the case if waves traveled from opposite ends of the cochlea and passed in the middle unless the waves were of nonnegligible magnitude only in a very short segment. Wever & Lawrence also measured the time required for a sound put in at one end of the cochlea to come out the other, and they found that it was very short, far too short for a slowly traveling wave.

Fortunately for the traveling wave theories, it is possible to show that neither of Wever & Lawrence's items of evidence is at all critical. According to the traveling wave theories, the compression wave influences the stiff basal part of the cochlear partition, and the traveling wave travels from that part. This is the case no matter at what location in the cochlea the vibratory force is applied, as Békésy (6) showed clearly with both models of the ear and human cochlear preparations. The interpretation of the cancellation experiment is therefore simple and direct. The vibrations applied at the two ends of the cochlea were adjusted until their contributions to the pressure difference across the basal part of the partition cancelled. There was then no traveling wave, and therefore no microphonic response. The interpretation of the velocity measurements is even simpler, for the velocity

measured was the velocity of the compression wave. According to all the theories, it goes approximately with the speed of sound in water. The important thing is not to get it mixed up with the slowly traveling wave to which it gives rise.

It is easy thus to dismiss the alleged evidence against the traveling wave theories, but doing so does not dismiss the parallel-resonator picture of the cochlear process. The pressure difference across each segment of the cochlear partition does of course act upon that segment, and that segment does of course respond in the way determined by the impedance presented to the driving pressure. The question is, to what extent is the driving pressure associated with the compression wave that reaches all parts of the cochlea almost simultaneously, and to what extent is it due to the traveling wave that passes more slowly along the partition-fluid transmission line.⁵

A direct and almost unambiguous answer to that question is given by observations made by Békésy (7). Békésy applied a very short pulse of sound to the oval window and, under stroboscopic illumination, watched the displacement of the cochlear partition at various distances from the oval window. He saw a moving wave of displacement, a traveling bulge. It started out with unmeasurably high velocity in the basal turn, for, as we have noted, the stiff basal part of the partition vibrates essentially as a whole. Then, as it continued along the cochlear spiral, it decelerated markedly. It reached the twentieth millimeter in about $1/5$ msec., the twenty-fifth millimeter in about 1 msec., and the thirty-third millimeter in about 5 msec. No movement of any sort was visible at the apical end of the cochlea until the traveling bulge reached it. That, of course, is precisely the behavior that the traveling wave theory requires. And it is not at all what the parallel-resonator theory says should happen. The only way to rationalize it, I think, is to argue that Békésy did not see the start of the displacement, as he reported he did, but, instead, perceived displacement only when the actual displacement was nearly maximal. On that basis, even though all the parallel resonators were set into motion at the same instant, he would have seen the high-frequency basal elements move first, the low-frequency apical elements move only after a delay.

However, another set of observations of Békésy's (9) cannot be brought into line with the parallel-resonance theory by any rationalization. These are the observations of the phase of the displacements produced by sustained sinusoidal excitation. For frequencies of 200 c.p.s. and higher, Békésy measured phase shifts from the base to the apex of the cochlea that were as great as 400° or 500° . But, if the resonant elements postulated by the parallel-resonator theory are simple resonators, the phase shift between one end

⁵ Békésy points out, in personal communication, that actually the question is complicated by the fact that there are four, or perhaps five, kinds of waves that can, and probably do, exist to some extent in a system like the cochlea. In his opinion, rapid damping of the others is responsible for the fact that only one kind of wave (the traveling wave) is visible in the motion of the cochlear partition.

of the parallel array and the other is limited to 180° . And there is no way to conceive of the resonators as other than simple resonators, for, in a given segment, all the parts of the cochlear partition move in phase. Therefore, Békésy's phase observations constitute definitive evidence against the parallel-resonator theory and justification for adopting the general notion of the cochlear process expressed by the current traveling wave or transmission-line theories.

It is worthwhile noting how far the theory of the action of the cochlea worked out by Zwislocki (91, 92), Peterson & Bogert (66), Ranke, (72, 73) and Fletcher (33) takes us from the classical resonance theory. First, unless the term *resonance* is defined so broadly as to include almost any kind of frequency selectivity, the current formulations are not resonance theories. It is true that each segment of the cochlear partition is thought of as having a resonant frequency, but this resonance is not essential for the production of selective oscillation of the partition and, in fact, the maximum amplitude of oscillation does not occur at the segment for which the resonant frequency is equal to the stimulus frequency. It occurs, instead, at a point between the stapes and the resonant segment. Second, the analytic action of the cochlea is now thought of as due to the interaction of the parts of the partition-fluid system, and not to the local "sympathetic" vibrations of the separate parts of the basilar membrane. It is reasonable, therefore, to consider the timing of the oscillations of adjacent parts of the membrane as of very likely significance in the excitation of the neurons of the auditory nerve. This makes things more interesting than they were when the classical theory restricted attention to the amplitude of vibration. Third, the whole question of damping has taken on a new and different interpretation. In the old theory, damping was incompatible with sharp resonance, and sharp resonance was thought to be necessary for good frequency discrimination. Now, although the law relating the amplitude and phase response of a system to its transient response is with us just as much as ever, it is seen that, instead of leading of necessity to broad spread of activity within the cochlea, damping is in large part responsible for the frequency-selective action that does occur. In short, the reciprocal relation that exists between the time and frequency domains does not, in the current theories, imply a reciprocal relation between the time and place domains. And, fourth, the new theories are in principle much more powerful. It is now possible, at least in principle, to describe the behavior of the cochlear partition, not only in response to pure tones, but in response to any combination of tones or to any precisely defined vibratory input. This has become an important consideration during the last few years during which complex acoustic stimuli have displaced pure tones in the psychophysical study of hearing.

The unidirectional component(s) of the electrical response.—In 1950, Davis, Fernández & McAuliffe (27) reported the existence of a unidirectional potential in the electrical response of the ear, a wave with approximately the same form as the envelope of the familiar microphonic response. The

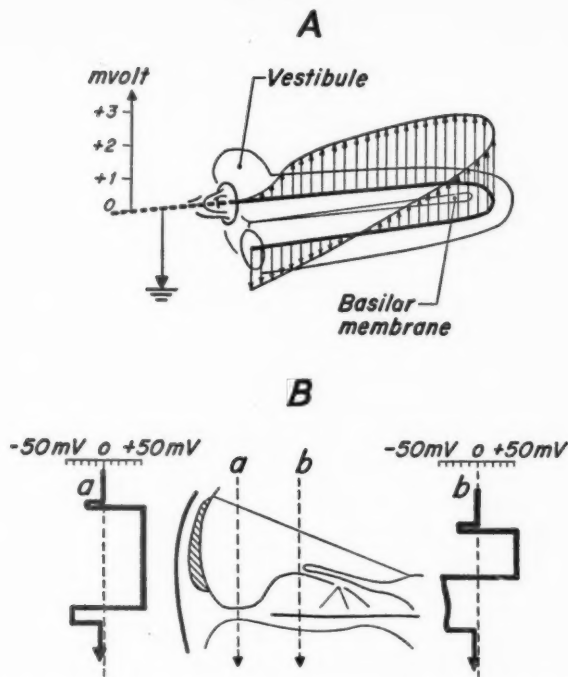


FIG. 1. A shows the D.C. potentials measured by Békésy in the perilymph along the vestibular and tympanic canals. B shows the potentials recorded by an electrode as it moved down through the cochlear partition along lines a and b. [From Békésy (10, 12).]

new component they called the "summing potential," suggesting an origin either in the excitatory process or in a microphonic process that plays a role in excitation. Independently, Lowy & Kemp (57) also observed a uni-directional (for short, D.C.) component.

Recently, Davis (26) and his co-workers found that the D.C. component reverses polarity when the guinea pig's oxygen supply is reduced, and reverses again if anoxia is allowed to progress. This suggests that the D.C. component may be made up of several parts. In Davis' current view, at least one of the parts may be neural in origin. If it is, it is either of unexpectedly short latency (0.12 msec. in one observation) or attributable to local potentials rather than nerve impulses.

The D.C. component, and the recorded nerve impulses and the A.C. microphonic as well, are influenced by the passage of a direct current through

the segment of the cochlea from which they are recorded [Tasaki & Fernández (86)]. Whether the magnitudes are increased or decreased depends upon the direction of the D.C. flow.

Békésy, also, is working on a D.C. component. His D.C. component is clearly of microphonic, as opposed to neural, origin. It may or may not prove to be related directly to the potential recorded by Davis and his co-workers.

Békésy (10) measured, first, the relative potential levels of various points within the quiescent cochlea. He found, between a point near the stapes in the vestibular canal and other points in the vestibular and tympanic canals, the potential differences shown in Figure 1A. They are of the order of a millivolt. Then, entering the cochlear partition, Békésy (12) found much greater resting potentials. The variations in potential along two paths down through Reissner's membrane, through the endolymph, through the basilar membrane, and out into the tympanic canal, are shown in Figure 1B. Even in silence, the cochlear partition is electrically active.

Békésy's D.C. potential is influenced by acoustic stimulation. The D.C. potential difference between the round window and an indifferent electrode drops when a tone is turned on. If the tone is not too intense, the "D.C. fall" is proportional to the amplitude of the A.C. microphonic picked up by the same electrode on the round window.⁶

Békésy's observations bear on a question that has been debated for years. Does the microphonic response of the cochlea result from direct transduction of mechanical energy into electrical energy, or does it depend upon the release of energy, under the control of the stimulus, from sources within the cochlea? Two lines of evidence lead Békésy to conclude that the second alternative is correct.

First, if the energy of mechanical oscillation were transformed directly into electrical energy, a decrement in the appearance of electrical energy would have to be matched by a decrement in the disappearance of mechanical energy. But an experiment indicates that such is not the case. Suddenly lifting a tiny iron ball from Reissner's membrane sets the cochlear partition into transient oscillation. The electrical response is recorded before and after reduction of the oxygen supply to the guinea pig. The form of the damped wave is unaffected by the anoxia, but the amplitude is reduced to one-fourth its former value. The fact that the form of the damped wave is not changed indicates that the rate of disappearance of mechanical energy is still the same.

⁶ I have just seen the galley of another paper by Békésy (13). He first shows that Reissner's membrane and the basilar membrane effectively isolate the cochlear partition from the rest of the cochlea. He then measures both the D.C. and A.C. microphonic response with a fine electrode that moves downward through the cochlea, piercing the various membranes, cells, and ligaments. Except for the insulating membranes, all the components generate some electrical potentials. The first-order potentials, 5 to 10 times as large as the others, come, however, from a restricted source in the organ of Corti.

The fact that the amplitude is lower indicates that less energy is showing up in electrical form. *Q.E.D.*

Second, if one can demonstrate that more energy appears in electrical form than disappears from mechanical form, he can thereby destroy the possibility of direct transduction. Békésy did this by selecting the simplest case. He produced a static deformation of Reissner's membrane by pressing down on it with a needle, at the same time measuring the force on the needle and the displacement of the membrane so he could calculate the amount of mechanical work done on the system. On the electrical side, he measured the potential difference between two points in the cochlea separated by a known resistance. The deformation increased this potential difference and, therefore, increased the amount of electrical power being dissipated in the resistance—by a calculable amount. It turned out that the extra dissipation of electrical power, sustained for 1 sec., amounted to more energy than did the work done by the needle. Again, *Q.E.D.*

Finding that the process that produces the microphonic response is not passive does not mean that the action of the cochlea is not like that of a microphone. It means only that the cochlear microphone is not a passive microphone, is not—for example—like a piezoelectric crystal. A better analogue would be the recently developed vacuum-tube microphone: the vibration of the diaphragm is transmitted mechanically to the control grid of a vacuum tube, and the vibration of the grid modulates the flow of an electron current that is supported by a battery or power supply.

The space-time pattern of the microphonic response.—Tasaki, Davis & Legoux (85) recorded microphonics through differential leads, pairs of 20- μ electrodes inserted into the cochlea (one electrode in the vestibular canal, one in the tympanic), and found that pickup is restricted mainly to a segment of the cochlear partition about 1 mm. long. Using a number of such pairs of electrodes, they determined the amplitude and phase of the electrical response as a function of stimulus frequency and electrode location. Their measurements are relative (response in basal turn held constant by adjusting stimulus intensity), but in other respects they are directly comparable with Békésy's measurements of displacement. The qualitative features of the electrical picture are reasonably similar to those of the mechanical picture.

Electrical response of the auditory nerve.—The neural component of the electrical response to an acoustic click, picked up by an active electrode near the round window, is a complex phenomenon. Rosenblith & Rosenzweig (76) found that they could record it without contamination by microphonics if they placed the electrode in one of a number of locations that were, for the microphonic response to a click, null-response locations. Moving the electrode from one such location to another accented some parts of the neural response and attenuated others. Usually, the neural response consisted of two prominent spike-like parts.

McGill (58) and McGill & Rosenblith (59) studied the behavior of the

first part of the neural click response, recorded at the round window, as a function of the intensity and polarity of the stimulus pulse and of the intensity of a conditioning pulse presented prior to the stimulus pulse by a variable interval. Their results led to the formulation of a statistical model. According to the model, the part of the response under study is the summated effect of essentially simultaneous discharges in many independent neural units, each of which has a threshold, an all-or-none response, and a recovery curve. By assuming two sets of neural units, one with a Gaussian distribution of thresholds and the other with an unspecified distribution, they accounted for the behavior of the response under the wide variety of conditions specified by their parameters.⁷

DETECTION AND DISCRIMINATION

Under this heading, which has been a main topic in the psychophysics of hearing, there is not in the work of the last year the material for a coherent story. There are, however, some interesting pieces:

The quantal nature of discrimination.—Corso (24) contributed another chapter to the debate on quantal discrimination. He determined psychometric functions for frequency and for intensity discrimination. In all, he obtained 70 functions, each based on roughly 1200 judgments. The critical questions were: (a) do the points fall along straight lines, and (b) is the smallest increment always reported just twice the largest increment never reported? Only 9 of the 70 functions passed the chi-square test of goodness of fit to the straight line, and only one of those came really close to the two-to-one ratio. Corso therefore rejected the quantum theory.

Taken at face value, Corso's procedure for arriving at a decision to reject the theory seems to be straightforward. However, we are left, I think, almost as we (probably) were beforehand: not convinced either way, and wondering what—unless it might be finding perfectly Gaussian or perfectly linear psychometric functions ourselves—might convince us. The difficulty is that the quantal model cannot be expected to hold if the observer's responses are influenced by noise, by change of set, by warming up or quieting down, or fatigue or boredom. Therefore, to disprove the theory, one must not only fail to get psychometric curves that meet the criteria, but he must convince his colleagues that he has squeezed all the non-

⁷ With this brief section, we leave the mechanical and physiological studies on hearing. The middle ear we can dismiss with a note that Payne & Githler (65) have destroyed various parts and amounts of the drumskin and noted the effects upon the electrical response, and that Fletcher (34) has demolished the old theory, well recounted only recently by Lawrence (50), that the drumskin and ossicles constitute an effective impedance-matching transformer. Fletcher showed that our insensitivity at low frequencies is due very largely to the extreme inefficiency of those coupling components. Not much has been done in the last year on the lower centers of the auditory system. Studies on the medial geniculate body and the auditory cortex are covered in the chapter by Dr. W. D. Neff. (See pp. 255-257.)

physiological error variance out of his measurements and has worked at the physiological limit. The finder of positive evidence, on the other hand, has an easier task, for most of us hope that things will turn out eventually to be simple, and seeing points fall neatly along a straight line makes us feel that we have had a good, clear look at the true mechanism in action. It is a shame that the quantum theory has such strong built-in self-protection.

Differential threshold for intensity.—Extending the work of Miller (60) and Harris (41), Pollack (69) determined the differential threshold for white noise as a function of the rate of alternation between the standard and comparison segments of the noise. As the rate of alternation increased, and therefore as the interval between the more intense segments ("bursts") decreased, the threshold remained constant at about 0.5 db until the interval between bursts was about 50 msec., whereupon the threshold started to increase. Pollack interpreted this fact as confirming Miller's conclusion (61) that it takes about 50 msec. for the activity, set up in the nervous system by white noise, to die down. At shorter intervals, the neural effects of one burst of noise pile upon the heels of the preceding burst and the fluctuations of intensity in the neural signal are less marked than they are when the intervals between bursts are longer. This, of course, would explain the change in the difference limen, for the stimulus differential would have to be increased to produce the same neural differential.

Differential threshold for frequency.—Harris (42) reported measurements of the influence on the differential threshold for frequency of the interval between the standard and comparison tones. This influence was surprisingly small, especially when the standard frequency remained fixed throughout the experiment. In that case, the threshold (about 4 c.p.s. at 1000 c.p.s.) was nearly independent of the interval for intervals from about 0.3 to 7 sec., and it was only a little over 7 c.p.s. at 25 sec. When the standard frequency was moved about in the range 950 to 1050 c.p.s., the threshold increased more rapidly as the interval was lengthened. It started near 4 c.p.s., reached 8 c.p.s. at about 15 sec.

These data add to the picture that has been taking form over a period of several years: the observer builds up a reference structure and judges with respect to it, not to the standard-comparison pair in isolation. The behavior of Harris' thresholds, the anchoring of "absolute" judgements, and possibly figural aftereffects and some phenomena of auditory "fatigue" reflect the dynamics—as yet by no means well understood—of that reference structure.

Masking.—It is a matter of some importance, for example in designing detection systems that employ aural presentation of signals to the operators, to know whether a listener gets, from a single presentation of a signal, almost all the information he is ever going to get out of that signal, or whether he can profit from repeated presentations. Schreitmüller (78) studied that question in a very simple case, the detection of a short segment of tone in the presence of random noise. He found that, when the same pulse was

repeated n times, each time in precisely the same sample of noise, the optimal value of n for detection was 2. The percentage of 10-sec. trials in which anything was detected increased a little as n went from 1 to 2, but it decreased again when 4 or 6 or 8 of the 10 1-sec. intervals contained a pulse of tone. On the other hand, Schreitmüller found the more pulses the better when the successive samples of noise were different. Even though they were successive samples of a noise with constant statistical parameters, the samples were different enough for the listeners to take some advantage of the multiple presentation. The advantage of eight pulses over one was about the same as that provided by turning the background noise down 3.5 db. These results have a rather striking implication: there must be very little fluctuation in the detection process within the auditory system. If there were, the best one of eight looks at the stimulus would be much better than just one look taken at random.

Absolute thresholds of animals.—Ash's review (4) on the sensory capacities of infrahuman animals summarized the product of over 50 papers on animal hearing. The product is hardly commensurate with the quantity of effort, mainly because much of the work suffered from lack of precision of acoustic measurement and from lack of absolute reference intensities. There is, however, a tendency for the sensitivity curves of Ash's graph that summarizes the most important work—determinations of the absolute thresholds of various animals—to follow the rule: animals are less sensitive than man at low frequencies, about the same in the middle range, and more sensitive at high frequencies. [Re high frequencies: In rodents, Schleidt (77) observed reflex responses to sounds ranging up to almost 100,000 c.p.s.] Since the animals were all smaller than man (and had smaller cochleas), this is in line with the implications of Békésy's (8) observations on the physical and mechanical characteristics of the ears of animals. The rat is shown so much less sensitive than man, however, that one would suspect a gross error if Jamison's (47) recent measurements of the rat's threshold did not roughly confirm Ash's curve.

Observations by Antrum & Poggendorf (3) show that, on the basis of power sensitivity, small fish are more sensitive than man at low frequencies, less sensitive at high. This is a reversal of the rule just mentioned, but the fish (having neither air around them nor cochleas within them) do not have the problem of matching impedances that, according to Fletcher, accounts for man's relative insensitivity at low frequencies.

SUBJECTIVE ATTRIBUTES OF SOUND

Just as engineers are beginning to express their results in mels and sones, and even to build meters that read directly in subjective units [Cudworth (25)], psychologists are finding cause for concern about the reliability and/or validity of the subjective scales.

Loudness.—A loudness scale is the relation between the subjective loudness of a sound and its level above threshold (2). Although for a long time

it was argued whether or not the subjective magnitudes are measurable, the problem in recent years has settled down to being one of the exact form of the relation, and even that question was regarded as sufficiently well answered [despite the divergence of such results as those of Laird, Taylor & Wille (49)] to warrant the adoption of a standard loudness scale (1). However, Garner (37) recently described the results of a thorough study of half-loudness judgements (on which loudness scales are chiefly based), and his results suggest that we have been talking and calculating, these last few years, with an unwarranted air of precision.

Garner's study showed that the variation among listeners in making half-loudness judgements is great. Comparing the two extreme listeners in the group of 18, he found that while one required only about 7 db attenuation to reduce a 1000-cycle tone to half loudness, the other required about 27 db attenuation. This difference was a stable difference between the listeners; it was not due to error of measurement. Despite the wide range of variation, it was of course possible for Garner to construct a loudness scale. His comparison of that scale with the American Standards Association scale challenges the growing tendency to use loudness scales as though they were meter sticks. At a loudness level of 10 phons, Garner's loudness was 0.2 sone and the ASA loudness was 0.0143 sone. At a loudness level of 100 phons, Garner's loudness was 12.6 sones and the ASA loudness was 90.2 sones. The discrepancy from one end of the scale to the other, therefore, is a factor of $(0.2/0.0143)(90.2/12.6)$, or almost exactly 100.

As a supplement to the method of fractionation (half-loudness judgements), the method of monaural-binaural comparison was used in the construction of most of the loudness scales. This method involves the assumption that a sound heard with two ears is twice as loud as a sound heard by one. But is it? Pollack (71) determined loudness scales for speech, one by fractionation, one by monaural-binaural comparison. At 15 db sound pressure level, one scale has an ordinate of 0.16 sone as against 0.02 for the other scale. At 100 db the corresponding values are about 35 sones and 700 sones. The discrepancy factor⁸ here is over 200.

Pollack (70) also determined a loudness scale for white noise. He used half-loudness judgements, monaural-binaural comparisons, and, in addition, a substitution or transfer method in which the noise was matched in loudness to a 1000-cycle tone and the magnitude of the loudness then read from the loudness scale for the tone. The equal loudness matches for a given

⁸ It is reasonable to suppose that the listener's long-standing habit of paying attention to the meaning of speech might influence in some way his conception of its loudness and pitch. Indeed, observations by Cooper, Liberman & Delattre (23) indicate that some of their synthetic sounds change markedly in subjective character when, as sometimes happens, the listener suddenly starts hearing them as speech after having heard them merely as complex tones. But, if they both get at the same subjective attribute, why should the fractionation and monaural-binaural comparison procedures yield such divergent results for speech?

listener were consistent and repeatable, but differences among listeners were as great as 30 db. At 100 db sound pressure level, the discrepancy between the loudness computed from half-loudness judgements and monaural-binaural comparisons and the loudness derived from the loudness scale for the tone was about 700 sones to 140 sones.

These evidences of variability do not indicate that loudness is not a legitimate attribute of auditory sensation. They do not disallow the possibility of determining a scale of loudness representative of the central tendency of a population of listeners. On the other hand, it is important to realize how great the differences are among listeners and among loudness scales. In practical work, where the great value of being able to quantify the reactions of human beings tempts one to say, for example, that such-and-such a treatment reduces the loudness of noise by a factor of 5, it makes a real difference whether the 5 is 5 or 25. And in theoretical work, where the great value of being able to accept or reject a theory on the basis of a quantitative prediction tempts one to rest an argument on the evidence, for example, that two ears are twice as loud as one, it makes real difference whether the two is two or the square root of two. Particularly important, I believe, are two notions that are brought back to our attention by the discrepancies among the loudness functions: (a) loudness is to be understood probably not so much in relation to any simple neural mechanism [number of nerve impulses reaching the brain in unit time (32, 67), total excitation in an auditory center (19)] as in relation to the development of the concept of loudness in the verbal habit system of the individual; and (b) factors such as set, attitude, and expectation are perhaps as important as intensity in determining the subjective reactions of a listener. Perhaps related to this second point, and certainly relevant to the use of the Fletcher-Munson procedure (35) in evaluating industrial noise problems, is Pollack's (68) finding that the loudness of an interrupted noise cannot be calculated from its power density spectrum. A noise may sound louder when it is turned on and off than it does when it is left on all the time.

Pitch.—Apparently expecting better consistency with pitch than the experiments just described found with loudness, Morgan, Garner & Galambos (64) asked the question, "Precisely what is the relation between pitch and intensity?" They found, however, that "no two sets of data were alike. The functions for one ear might go up with intensity while those for another ear might go down." Excellent statistical analysis and display of their data enabled them to show that, in so far as central tendency was concerned, their results confirmed the direction of variation of pitch with intensity of the earlier functions of Stevens (84) and Snow (79)—enabled them to show that without submerging the main result: that everyone has his own, personal pitch versus intensity function.

Pitch has another difficulty. There is not agreement that it is just one attribute. Opposing a duplex conception (28, 54) of pitch, Garner (36) gave an analysis of the difficulty introduced into the discussion of auditory sub-

jective attributes by the fact that the range of frequencies over which we can experience "intermittency" overlaps the range of frequencies over which we experience "pitch." He pointed out that, in other modalities, intermittency is perceived in approximately the same frequency range as in hearing, which fact "should remind us that intermittency can be experienced and that it does not have to be called pitch." Certainly it does not have to be called pitch, for a rose by any other name. . . . The trouble is, we do call pitch—whenever we listen to music in the lower or middle registers or to any other sounds that afford us the opportunity to check up on the basis of our perceptions—the attribute that is related to periodicity or intermittency. What about the attribute that is related to the place of maximum displacement? Well, unless we adopt the terms "tone height" and "chroma" or "buzz" and "body," we call it pitch, too. That is one of the confusing things about the subjective attributes.

HEARING SPEECH AND SPEECH-LIKE SOUNDS

In previous volumes of the *Annual Review of Psychology*, the hearing of speech was treated very briefly. This year there are developments that warrant more extensive description.

Synthetic speech.—A steady vowel sound has a line spectrum consisting of the first 50 or so harmonics of the fundamental frequency. The amplitudes of these harmonic components are governed by the (variable) transmission characteristic of the vocal tract, and it usually turns out that there are three main intervals along the frequency scale in which the amplitudes are relatively high. The concentrations of energy are the formants.

In order to study the effect of varying the characteristics of isolated formants, K. N. Stevens (81, 82) simulated them by applying impulsive excitation to a simple resonant circuit. He measured, for single pulses and for series of about 45 pulses, the differential threshold for formant frequency (i.e., for resonant frequency of the tuned circuit) and for coefficient of damping. As the damping increased, the sensitivity to changes in frequency decreased—more markedly for low-frequency formants than for high-frequency formants, as the relation between damping and bandwidth, considered together with the function relating Fletcher's critical bandwidth to frequency, would lead us to expect. The difference limen for damping was about as large as the differences in damping among actual vowel formants, but the difference limen for frequency was much smaller than the differences that exist between corresponding formants of vowels that are neighbors in the vowel triangle. The latter fact Stevens interpreted as reflecting either (a) a considerable factor of safety in speech perception or (b) the action, in speech perception, of a part or mode of the auditory system that is different from the part or mode that mediates the discriminations in psychophysical tests.

Approaching actual speech a step closer, Stevens explored the influence of the presence of a second formant upon the discriminability of small changes in the first. There was little degradation except in a case in which both form-

ants were at high frequencies. Stevens has not yet made tests with three formants, but observations with three-formant speech synthesizers indicated that the procedure of synthesizing speech sounds by applying controlled excitation to three variable resonant circuits is reasonable. Two such synthesizers made clearly recognizable vowels and fair consonants.

Fant's work (31) on the theory of speech production is significant for the study of speech perception. Fant analyzed the action of the vocal mechanism in terms of the excitation function (a train of vocal pulses in the case of vowel sounds, fricative noise in the case of some consonants, etc.) and the transmission function of the system of throat and mouth cavities. He showed how the formants, which are basic elements in an acoustical analysis, are related to a few geometrical parameters of the vocal tract. These geometrical parameters give us a new basis for the analysis and description of speech sounds.

In an extension of Fant's work, Kasowski (48) and K. N. Stevens (83) constructed an electrical analogue of the vocal tract, an elaboration of an earlier device built by Dunn (29). It consists of generators of excitation functions and a variable transmission line. The latter consists of 35 cascade filter sections, each of which represents a short length of tube corresponding to a segment of the vocal tract. By adjusting the circuit to produce a clearly recognizable speech sound—it makes good consonants as well as good vowels—one can obtain directly a description of that sound in Fant's terms.

The change in our conception of speech production is similar to the change in our conception of the action of the cochlea. For models consisting of a number of resonators excited in parallel, we are substituting models consisting of a number of resonators connected in cascade, with the excitation traveling down the chain. An important corollary of this change is that we no longer identify a formant with a single resonator (e.g., back cavity of vocal tract), just as we no longer associate a maximum in the displacement of the cochlear partition with the action of the individual segment of the partition that is in resonance with the stimulus frequency.

The transmission-line models are not yet far enough developed, however, to rival the parallel-resonator models when it comes to flexibility and convenience of use. The sound spectrograph is still the basic analytic tool, and its complement, a synthesizer of sound waves from spectrograms has opened up extremely interesting possibilities in the study of speech perception. This device, developed in somewhat different forms by Cooper and his co-workers (21, 22) and by Vilbig (88), makes it possible to produce arbitrary revisions of the intensity-frequency-time pattern of a sound, or even to synthesize an entirely new sound by painting on a transparent strip of film.

Playing back synthetic spectrograms, Liberman, Delattre & Cooper (53) found that a sound usually heard as "p" when followed by "i" or "u" was usually heard as "k" when followed by "a." If confirmed in tests with actual speech, this finding will upset one of the basic notions about the phoneme. Whether regarded as a class, a target, a bundle of features, or the smallest

linguistic unit critical to the identification of a morpheme, the phoneme has quite generally had associated with it the idea that its determining features were within. Here is a case in which a critical condition for the identification of a consonant resides clearly outside the consonant.

Cooper, Liberman & Borst (22) explored the interconversion of audible and visible patterns somewhat simpler and more amenable to control and specification than speech. They painted circles, squares, triangles, etc., on the film and had subjects note similarities and differences among the audible, as well as the visible, patterns. Some of the invariances that characterized the visual perception also appeared to characterize the auditory perception, but there were exceptions. All the triangles had something in common, aurally as well as visually, but a visual diamond and a visual square were much more alike than their auditory counterparts.

Distorted speech.—In a study of public address systems, Beranek, Radford, Kessler & Wiesner (14) found that naturalness was not adversely affected if the frequency components of speech below 400 c.p.s. were attenuated. On the other hand, intelligibility was not impaired, even in highly reverberant auditoriums, if the low-frequency components were amplified, provided the high-frequency response was good out to 4000 c.p.s. In extending these results, already established for headset listening, to auditoriums, they developed a useful variation of the articulation test: listeners seated in the audience wrote down the final words of the sentences of the speech being delivered, and their records were scored against tape recordings of the signal picked up by the microphone.

In an effort to dissect out of the problem of room acoustics a piece amenable to laboratory measurement, Haas (38) studied the influence of single echoes upon intelligibility. The echoes were delayed by various intervals and were amplified or attenuated by controlled amounts relative to the direct speech wave. When the interval was less than 30 msec., the echo made the speech sound louder and changed the timbre, but was not perceptible as an echo and had no effect upon intelligibility. However, longer intervals yielded detrimental effects upon intelligibility and, at a critical interval that depended upon the intensity of the echo and other factors, the speech became practically unintelligible.

Auditory control of speaking.—Continuing interest in the roles played by hearing in the learning of speech, the maintenance of speech habits, and in the adaptation of speech to ambient acoustic conditions is reflected in papers by Huizing & Pollack (45), Hudgins (43), and Hardy, Pauls & Bordley (39). And a systematic extension of the Bell Telephone Laboratories' study of the "side-tone reflex" is under way in a series of experiments by Black and his co-workers (15, 16, 17, 55, 56). But the dramatic development in auditory control of speaking is the delayed feedback effect.

Delaying the side-tone, as almost everyone now knows, disrupts the talker's speech. The talker blocks, slurs, stretches out his speech sounds until he hears them come back to his ears. This effect was noticed almost simul-

taneously all over the country when tape recorders with delay in the monitoring channel made their appearance. However, Lee (51, 52), Black (17), and Fairbanks & Jaeger (30) had already been studying the effect with apparatus that provided variable delay. Their results indicate that the disruption becomes marked (if the side-tone gain is high enough) as the delay is increased to about 0.1 sec. It is not evident from the data that there is a clear maximum of disruption, but the interval of critical delays may be related to the durations of speech units or to the much-contraverted perceptual time base. Implications of the effect for stuttering, stammering, expressive aphasia, and other abnormalities involving speech have of course been suggested. Sufficient data are not yet available for one to evaluate the suggestions or to put together a definite picture of the delayed feedback effect, but the study is proceeding, and it appears that it may in due course lead to an understanding of what is certainly one of the most interesting biological feedback mechanisms.

A practical application of delayed feedback has already been made. Azzi (5) and Tiffany & Hanley (87) found that, since persons with normal hearing cannot entirely overcome the disruptive effect, a delayed side-tone test made an excellent trap for malingerers.

Theory of speech perception.—The progress in the last year toward a theory of speech perception was the sharpening of three ideas.

(a) Cherry (20) considered the perception of speech from a communication engineer's point of view and emphasized the inductive, statistical nature of the process. His functional model is a sequential decision-maker, operating on the principle of Bayes' Theorem in the way described by Woodward & Davies (90). The noisy received signal consists of elements in sequence, the linguist's phons. Each phon corresponds to, and should be identified by the listener with, a phoneme. The elements are chained sequentially under the influence of conditional probabilities. The process of reception (inverse-probability calculation) therefore takes into account both the new evidence and the a priori information contained in the statistics of the already received signal. As information continues to come in, the listener narrows down the possibilities until there is a sufficiently strong concentration of likelihood about one phoneme or sequence of phonemes to warrant the decision that it is the correct one. It is then stored, and helps to condition the calculations for the next decision. Beyond the stage of deciding among phonemes comes a second stage of deciding upon larger units of the message. The process is again essentially a calculation of what is most likely in view of the a priori probabilities (which may be only rough estimates or rankings) and the incoming evidence.

(b) Jakobson, Fant & Halle (46) elucidated the significance for speech analysis of the distinctive-feature theory of the phoneme. According to that theory, somewhat oversimplified, a phoneme is a set of binary distinctions or oppositions. For example, the "k" in king is not vocalic but consonantal, compact not diffuse, not nasal but oral, tense not lax, not continuant but

interrupted, and not strident but mellow. The "k" can therefore be represented by the binary digits 010100. It is different from every other phoneme in at least one digit (distinctive feature). Although Jakobson, Fant & Halle did not explicitly formulate a theory of the perceptual process, the implication is clear that one of the early stages of the process should involve detections of the distinctions, and that the identification of a phoneme should rest on the bringing together of the (approximately six) elementary decisions. One might call this a teletype theory of phoneme recognition, for conventional teletype receivers operate in almost precisely the way just described.

(c) Broadly speaking, speech perception (as well as other perceptual processes) seems to involve the comparison of a received pattern with stored, standard patterns. There are two basic ways of making the comparison. The received pattern can be held up against the standard so that the degree of correspondence can be measured, or it can be forced into or through the standard so that the degree to which it fits, or is tuned to, the standard can be determined. The first notion is closely related to the concept of correlation, the second to the concept of matched filtering and the convolution integral. The implication for auditory theory is that part of the nervous system must function in one of these two ways in recognizing the elements of speech and also in comprehending ideas. Examination of the properties of the two models suggests that the recognition of phonemes might involve matched filtering whereas the comprehension of ideas might involve correlation.

OTHER DEVELOPMENTS

Important work has been done on several topics I have not tried to cover. Problems of industrial and military noise, particularly jet aircraft noise, are becoming critical, and some excellent studies have appeared. Now is not the right time to summarize the picture, however; it will be much more complete next year or the year after. A similar situation exists in the application of information theory to psychophysics. There is a start, and there is a promise of much to come, but there is not yet a complete picture. In audiometry there were many papers, and in otology even more. There were valuable articles on auditory fatigue, on binaural hearing, on sensory prosthesis, on recruitment, and on noncochlear, nonspeech auditory theory.

Finally, I should like to mention one journal and two books. A new, international journal, *Acustica*, made its appearance. Associated with it is a German supplement, *Akustische Beihefte*. They will do much to get the backlog of European contributions to audition (40) into print. Miller's *Language and Communication* (62) brings together in one place contributions fundamental to a broad understanding of hearing, from phonetics, psychoacoustics, statistics and communication theory, logic, the psychology of individual differences, developmental psychology, the psychology of learning, and social psychology. In addition, it develops in detail the important role of contextual determination in speech production and perception. Stetson's *Motor Phonetics* (80) makes available a wealth of little known work on

the production of speech, much of which is clearly significant in relation to the understanding of speech.

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SOMESTHESIS AND THE CHEMICAL SENSES^{1,2}

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The title of this review has presented a difficulty. A previous reviewer in this series has spoken of the "minor" senses, while some authors have used the expression "other" senses. Some years ago, this reviewer described the chemical and visceral senses as the "neglected" senses. He has objected to the term "chemical" senses because similarity of stimulus has tended to obscure both the dissimilarity between taste and olfaction in central organization and the similarity between taste and somesthetic sensation in this respect.

The neurophysiological aspects of the topic are emphasized in this paper because the reviewer sees in this sphere signs of a confluence between the physiological and psychological disciplines. He also holds that the fashionable belief that psychology should not look for explanations in physiological terms is gauche, insular, and contrary to the history of science and to evidence.

The year has been marked by the appearance of three comprehensive works dealing with somesthesia and chemoreception. Since, in the minds of some, reviews and other secondary sources are currently overemphasized, two of these, Stevens' *Handbook of Experimental Psychology* (56) and Blake & Ramsey's *Perception: An Approach to Personality* (12), will not be discussed here. However, they are highly recommended. The third general work is a symposium arranged by Bard (7) on the "Patterns of Organization in the Central Nervous System." Section IV, consisting of nine papers, is devoted to the sensory systems including the diffuse thalamocortical system, and Section III is, in part, concerned with sensory pathways to the cerebellum. Both sections contain detailed reports of experimental work not yet published elsewhere, and are discussed below. A European symposium on "Chemoreceptors and Chemoreceptive Reactions" (44) also was published but is devoted to purely physiological considerations.

SOMESTHESIS

Mechanoreceptors.—The type of sense organ analysis introduced by Adrian & Zotterman³ some 25 years ago continues to yield dividends. During

¹ The survey of the literature to which this review pertains was completed in June, 1952.

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³ References to work falling outside the period of this review are not listed in the bibliography, but are usually found in the articles under discussion.

the past year, several studies of the exact nature of the specific stimulus for touch-pressure, thermal, and proprioceptive end organs, and of the contribution of the non-neural structures to the end organ process have appeared.

The abilities of certain sense organs and of their particular axons to maintain long-continued repetitive response to a constant stimulus (the reciprocals of adaptation and accommodation, respectively) are so similar they prompt the thought that adaptation of a sense organ is determined simply by accommodation of its axon. It was shown earlier by Gray and Malcolm that the pacinian corpuscles can adapt to a mechanical stimulus as rapidly as their axons accommodate to steady currents. To further the comparison between the two structures and their adequate stimuli, Gray & P. B. C. Matthews (22) determined the critical slopes of excitation for an increasing pressure stimulus applied to a single pacinian corpuscle and for an increasing electrical stimulus to its axon. (The critical slope is the rate of increase in a stimulus which is sufficient to excite.) While statistically the critical slope for the corpuscle was significantly greater than that for the axon, both were extremely rapid, and the difference was not quantitatively significant in view of the large differences between various classes of receptors in this respect. Thus, the pacinian corpuscle adapts rapidly and is served by a rapidly accommodating axon. Consistently, both gave rise to a shorter train of discharges to a constant stimulus than do most other receptors and their axons studied so far. The general weight of this study points away from a specific end organ process, as opposed to axon properties, in explaining certain features of sensory end organ properties. It is paradoxical that the pacinian corpuscle, the most highly encapsulated end organ known, should react in this way and, in passing, it may be noted that the mesenteric pacinian corpuscle seems poorly suited to signaling the sustained pressures or slow transients which might be produced by visceral distension.

The studies of Gray & Malcolm (21) on the touch receptor of the frog's skin were similarly oriented though single unit recording was not possible. They used mechanical pulses of controlled slope, duration, and magnitude, recorded photoelectrically on the same film with the resulting axon action potentials, to examine the mechanoreceptors of the skin in the traditional manner of axon studies employing electrical stimulation. A qualitative comparison could thus be made between receptor and axon. Only a considerable increase in stimulus strength over threshold (1.4 to 5.7 times) generated a second impulse and, as with other rapidly adapting sense organs, "on" and "off" effects were seen. As in the familiar strength-duration relationship for nerve excitation, the threshold amplitude of a mechanical pulse was inversely related to pulse duration. Similarly, the latency of response was qualitatively related to pulse amplitude (duration infinite) as it is in electrical excitation of nerve. Summation effects were shown to exist between a constant, conditioning, mechanical deformation of subthreshold strength and a mechanical testing pulse of brief duration. Up to 40 msec., the threshold stabilized at 35 per cent of the threshold obtaining when no conditioning stimulus was

applied. As might be expected, there were quantitative differences between axon behavior and that of the touch receptors (which in the frog are mostly unencapsulated), but, in general, the two structures appear to behave in the same way.

To isolate single pressure units, Hensel & Zotterman (33) recorded from a thin filament of the lingual nerve after the number of active units had been reduced by stigmatic anesthetization of the tongue. Their object was to study the responses of mechanoreceptors to cold. Specific pressure receptors, those having axons of large diameter, were unresponsive to cold. Other pressure receptors, served by smaller fibers, responded to cold, but only during a sudden, large fall in temperature—e.g., 41° to 29°C. That the nerve fiber was directly stimulated or that the discharge was secondary to vasoconstriction seem adequately ruled out in these experiments. However, it is just possible that cooling potentiates pressure from the thermode rather than acting as a specific stimulus.

Proprioceptors.—Kuffler & Hunt (42) have drawn together their provocative work of recent years on the muscle spindle. The small (3 to 8 μ diameter) anterior root fibers innervate only the intrafusal muscle fibers of the muscle spindle (Matthews' type A end organ) and not the ordinary muscle fibers that make up the bulk of the muscle. Moreover, in the intrafusal muscle fiber, the fine fibers' stimulation produces only a local response which builds up on repeated stimulation at appropriate time intervals but remains nonpropagated. The fine fibers are an agency by which a central influence on the muscle fibers interacts with tension-stretch changes in the spindle to determine the afferent discharge to the spinal cord. Kuffler, Hunt & Quilliam (43) were able to study single, fine nerve fibers in mammalian preparations by stimulating them through subdivisions of ventral rootlets. Impulses in fine fibers conducted at 15 to 55 m./sec. or less did not cause detectable muscle contraction or propagated muscle action potentials, which appeared only when the larger fibers with faster conduction times were stimulated. Approximately one-third of the anterior root fibers are in the fine category. The differentiation in respect to type of muscle fiber innervated, and hence to contraction production, is absolute in the mammal, but only relative in the frog.

Kuffler, Hunt & Quilliam's preparation was a single, small afferent fiber, its muscle spindle, and the spindle's afferent fiber. Stimulating the fine efferent fiber increased the spindle's afferent discharge without activating any ordinary muscle fibers. Despite considerable variation between preparations, two major factors influencing the spindle discharge were determined: (a) external muscle tension which increases discharge and (b) the frequency and total number of small fiber impulses reaching the spindle. When the muscle was slack and the receptor quiescent, applying trains of 4, 9, and 14 stimuli to a small efferent fiber caused 1, 3, and 8 spindle afferent discharges. A steady stretch increased the so-called "resting" or base line discharge and also markedly increased the spindle discharge to small fiber stimulation. The receptor mechanism is clearly able to summate the external

stretch stimulus and the centrogenic stimulus resulting from central discharge to the intrafusal fiber.

A reciprocal frequency-time relationship exists. External stretch yielded an initial high-frequency discharge which slowed down; whereas the effect of the centrogenic stimulus was the reverse. Repetitive fine fiber stimulation discharged the spindle at increasing frequency and eventually drove the spindle, establishing a one-to-one relationship between the fine fiber and spindle afferent discharges. The graded, nonpropagated response induced in the intrafusal elements by fine fiber activity makes the muscle spindle more sensitive to stretch.

A third influence, brought out in an earlier paper by Kuffler and his co-workers, and still earlier by Matthews, is the contraction of the ordinary (extrafusal) muscle fibers surrounding the spindle in response to large efferent fiber discharge. This interrupts spindle discharge to stretch. However, ordinary muscle contraction does not interrupt the spindle under tension when driven by small fiber stimulation, provided shortening is prevented.

With these studies, knowledge of the proprioceptors is approaching the stage where it should explain reflex and psychological responses. One step in this direction is the finding that the small efferent fibers participate in a variety of reflex discharges (42). Their discharge is subject to reflex inhibition, and they are continuously activated reflexly from skin and muscle afferents even "at rest." The reflex efferent discharge could act to maintain the discharge of the afferent by compensating for spindle slackening during contraction which might serve to give an awareness of movement in process. Against this theory is the evidence showing that shortening terminates spindle discharge even in the face of small fiber activity. The effectiveness of this activity under isometric conditions suggests a postural role when the antigravity muscles are contracting isometrically or are actually being stretched. The basic unit of posture is the stretch reflex which maintains a constant position of the body in space in the face of varying environmental forces. The isometric response in a Liddell-Sherrington myotatic reflex preparation is accurately proportional to imposed stretch. It has always been difficult to visualize a stretch afferent which discharges to stretch and continues to discharge when the intrafusal muscle fiber contracts. The existence of a separate fine fiber innervation eliminates this dilemma while permitting an intensification of the sense organ response.

In terms of the "feed-back" analogy, the fine efferents to the spindle contribute "loop gain." If the difference between the environmental force tending to close a joint and the contraction of the opposing extensor muscle is the "error," the sensitization of the spindle to the stretch would increase the reflex discharge to the exterior muscle more brusquely and reduce the error. Oscillation would be prevented because any shortening tends to neutralize the small fiber effect.

Cold receptors.—After something more than a hundred years, Weber's theory that temperature change and not temperature per se is the stimulus

to the cold receptor has been directly examined, and apparently disproved. In 1936, Zotterman successfully recorded axon action potentials originating from thermal receptors. Meanwhile, Hensel, from psychological experiments involving intracutaneous temperature measurements, concluded that cold or warm sensation is still present when intracutaneous temperature is constant. These two authors have now joined forces and, in a series of six studies (28 to 33) combining the Adrian-Zotterman technique and intracutaneous temperature measurements applied to the tongue in the cat and dog, have reinvestigated the general problem of temperature sensation.

Cold receptor impulses are distinguished from impulses from contact receptors by their smaller spike size, slower conduction rate, and lesser adaptation (29). Any temperature gradient existing in the steady state was determined by measuring surface, intracutaneous and subcutaneous temperatures ($0.02^{\circ}\text{C}.$), although receptors are so close to the surface of the tongue that their temperature is almost the same as the surface temperature. When water flowing through the thermode was changed from $37^{\circ}\text{C}.$ to $13^{\circ}\text{C}.$, the impulse discharge reached its maximum frequency in 1 to 2 sec. and then fell, approximately exponentially. After 70 sec., the tongue surface and the tissue at the depth of the cold receptors having reached a constant temperature, the discharge was maintained at a constant rate (*ca.* 25/sec.). This discharge disappeared immediately after rewarming the tongue. A steady discharge from the cat's cold receptor was recorded after 70 min. of cooling to $20^{\circ}\text{C}.$ These experiments do not mean that temperature change is not an effective stimulus, but that it is not a prerequisite to stimulation. In the steady state, the receptor responds to a constant thermal stream, and the specific stimulus could be either the temperature gradient or the absolute temperature. Response to the latter could occur if the stimulus energy is not transformed into "physiological energy," and if excitation depends on some internal physiological process dependent on temperature. Many physiological processes depend on temperature, but the relationship is usually the reverse, cold reducing the rate of physiological processes. This difficulty is not insurmountable and actually certain neural processes—e.g., the efferent posterior root discharge—are augmented by cold.

In another study, Hensel & Zotterman (32) determined whether this steady state discharge is caused by a steady thermal stream—i.e., temperature gradient. They point out that "temperature gradient" may mean either the speed of temperature change ("temporal" temperature gradient) or the slope of the intracutaneous temperature fall ("spatial" temperature gradient). Several previous investigators have shown that it is the fact of the gradient (in other words, simple warming or cooling the receptor) that is important, not its direction (from without-in or within-out).

In a cat's tongue preparation containing active fibers only from the upper surface, cooling the lower surface discharged receptors after an interval of 1.5 to 3.0 sec. This discharge continued after the lower surface was rewarmed until the warmth had time to penetrate to the receptors. Stimulation does

not, therefore, depend on the direction of the gradient across the sense organ. However, given the same temperature differential between the upper and lower surfaces, the rate of discharge from the upper surface is much higher if the upper surface is the colder. This holds for experiments in which circulation in the tongue was stopped. Temperature per se rather than slope appears to be the adequate stimulus since the latter would be similar in the two cases.

Many of the objections to using penetration time to determine thermoreceptor depth are met when the first occurrence of potentials in the axons of receptors are used in the place of the psychological response. Hensel, Ström & Zotterman (28) used rectangular temperature pulses and measured the time from onset of pulse to first spike. This is chiefly penetration time since receptor latency and fiber conduction time are relatively small. They made up only .005 sec. of the .023 sec. elapsing before the first spike caused by a temperature drop from 38°C. to 15°C. appeared in the axon. If a thermal conductivity equivalent to water is assumed, penetration time gives the depth of the first responding receptor. A mean depth of 0.18 ± 0.04 mm. was obtained from many determinations in the cat. This places the receptor at the subepithelial level within and just at the base of the papillae, and just superficial to the vascular plexus.

By observing single units, Hensel & Zotterman (30) established the relationship between temperature stimulus and frequency of response. When the temperature was decreased from 34°C. to 32°C., one of two fibers under the electrode increased its firing from 9 to 35 impulses/sec. After 1 min. this fiber reduced its firing to 9.3/sec., which it maintained virtually unaltered for 15 min. Surprisingly, a cold receptor was observed to fire at about 1 impulse/sec. at 40.5°C.—i.e., above blood temperature. The plateau frequency of discharge reached by the cold receptor adapted to various temperatures, when plotted against stimulus temperature, is low at blood temperature, rises steeply to reach its maximum at 30°C., and then falls off to disappear at -5°C. (cold anesthesia).

A temperature difference of 1°C. corresponds to a two-impulse-per-second difference in frequency which, in turn, is 20 per cent of maximal frequency. This is a relatively limited range of intensity grading. However, the temperature yielding the maximum frequency varies for different single receptors. Through a statistical process, the relationship between the summed frequency of discharge in a preparation containing 10 to 20 receptors and temperature within the range of 38° to 20°C. is close to a straight line and the frequency varies from 0 to 100 impulses/sec. Therefore, a group of cold receptors can act much like a thermometer registering approximately in one-fifth degree steps.

When the temperature of a cat's tongue preparation was held at 40°C., no impulses were set up in the axon of the receptors (31). Although applying a 1:10,000 solution of menthol did not change tissue temperature, a strong,

steady discharge with the spike height characteristic of cold receptors ensued. Warming promptly caused these spikes to disappear, indicating that they were, in fact, derived from cold receptors. Moreover, it indicates that menthol is not actually an "inadequate" stimulus for cold receptors, but that menthol shifts their reactivity to the warm side—i.e., chemical sensitization rather than stimulation of the receptor. The threshold concentration for menthol lies between 1:1,000,000 and 1:5,000,000. Hensel & Zotterman suggest that the smallness of the effective concentrations of menthol is best explained by hypothesizing an action on some enzyme system. Such an enzyme system is also a convenient explanation for the dependence of discharge on temperature state rather than an energy change in the receptor indicated by the studies reviewed above.

Pain receptors.—In the study of pain receptors, the accent is not on single unit analysis with the axon discharge as the end point, used so successfully by Tower, but on an analysis based upon the psychological response as determined by the physical characteristics of the stimulus. (However, one phase of Tower's single unit work on the cornea has been nicely confirmed by an anatomical method. Zander & Weddell (60) found that the area supplied by a single nerve fiber is at least as large as one quadrant of the cornea.)

The radiant heat method for determining pain threshold has met with varying success during the past few years. During the year, the originators of this method (24) and others (11, 57, 59) have examined the validity of measuring thresholds in terms of stimulus energy as opposed to the temperature of the skin imposed by the heat stimulus. In using this method to test pain thresholds of various skin areas, of patients of varying age and with varying degrees of vasomotor reactivity, and after administration of drugs with potential effects on skin temperature, a discrepancy between energy flux and skin temperature might easily arise. Noteworthy, too, is the revival of interest in electrical stimulation as a method for obtaining pain thresholds in man. This perhaps reflects the prevailing skepticism, not wholly justified, of the radiant heat method.

In summarizing their study of the radiant heat method for measuring pain thresholds, Birren, Casperson & Botwinick (11) struck a note of cautious optimism about the method. However, they criticized the practice of expressing the range of individual differences as a percentage of mean stimulus energy and pointed out that an absorption filter, like the skin itself, increases the mean stimulus value without necessarily affecting the individual variation. Their attitude toward individual differences contrasts interestingly with that of the originators of the method who, working in a medical environment, sought a "test" in which individual variation was small, the better to detect the pathological variant whose "normal" threshold can not be known.

The reliability studies are encouraging. On 50 subjects, the correlation between two threshold determinations on the same day was 0.91 with

stimulus energy 12.5 mcal.; the same correlation on the second day was 0.86; and there was no significant lowering of the threshold within or between the two days. Between day 1 and day 2 the correlation was 0.85.

Skin temperature changes from the stimulus source were countered by using a 2-min. interval between exposures. Skin temperature levels of 85°, 90°, and 95°F. significantly affected the threshold, and Birren *et al.* conclude that even on the forehead, chosen for its relatively stable and uniform skin temperature, normal temperature variations and fluctuations induced by drugs might afford nonsensory influences on the pain threshold.

Hardy, Goodell & Wolff (24) have also studied the effects of skin temperature on the pain threshold determinations. An exact inverse linear relation between pain threshold and skin temperature was found. Either cooling or warming the skin 10°C. resulted in a threshold change amounting to roughly 200 mcal./sec./cm.² A formula is furnished for correcting determinations for skin temperature: Pain threshold = $200 + 20(34^\circ\text{C} - T_s)$ where T_s is skin temperature in degrees centigrade. This is recommended for skin areas other than the forehead, where stabilizing the room temperature between 20°C. and 30°C. is considered sufficient.

When cooled skin is warmed and then heated, the amount of radiant energy necessary to produce pain falls linearly and, by extrapolation, zero threshold is reached at 44.9°C.—i.e., no heat from the instrument would be required. Wertheimer & Ward (57) confirmed this intersect using somewhat different methods. Burning pain was used as the end point instead of pricking pain. The skin temperatures at threshold plotted against exposure time necessary to reach end point extrapolated to 44.1°C. as compared with 44.9°C., even though only a narrow range of skin temperature (26° to 35°C.) was used. Both studies agree that the temperature of the tissue, whether reached rapidly or slowly, is the critical stimulus for pain end organs. Histological evidence was cited which shows that temperatures of 44°C. to 45°C. are critical for reversible tissue damage in human skin. Thus, these experiments support the Sherrington-Lewis thesis that the adequate stimulus for nociceptors is tissue damage. Skin temperature affects the threshold for pain, warmth, and cold differently. This suggests that the specific stimuli to thermal receptors and to pain receptors are quite different, and that the sense organs are different.

Employing short (1 μ) and long (4 to 5 μ) infrared radiation delivered to large surfaces on the back or forehead, Whyte (59) studied the heat-pain threshold in terms of skin temperature. [The requirements for adequate measurement of skin temperature in the presence of radiant heat are described in a companion paper (58).] Accuracy within 0.5°C. was obtained. Two end points were used, "pricking" pain and "intolerable" pain. The threshold end points were reached at a constant skin temperature regardless of intensity of radiation, duration of exposure, initial temperature, and whether the skin was blackened or not. While short-wave infrared radiation yielded slightly higher thresholds than long-wave, the difference was statis-

tically insignificant. The 3.5-sq.-cm. area used by Hardy, Wolff & Goodell was superior to a 16-sq.-cm. area.

In 200 observations, the mean skin temperature for pricking pain was $46.58^{\circ} \pm 0.03^{\circ}\text{C}$. In contrast with the generally accepted work of Wolff *et al.*, neither aspirin nor morphine significantly raised the pain threshold in terms of skin temperature. Whyte suggests that these discrepancies in drug studies may be due to secondary effects of the drugs on blood flow, sweating, or lower initial temperatures. He also argues strongly for stating radiant heat pain thresholds in terms of skin temperature rather than the amount of radiant energy.

These three studies, taken together, all show that temperature rather than temperature change is the specific stimulus for pain receptors, as it is for thermal receptors (see above). Beyond this point, they differ on whether the radiant heat method can be sufficiently standardized for the forehead, and whether thresholds can be determined in terms of radiant energy, for normal subjects, for aged subjects, and in drug or other studies that might change the condition of the skin. In view of these objections the results of drug studies to date should be re-evaluated.

After more than a decade of disinterest in electric shock as a means of testing pain sensibility (coinciding with the vogue for radiant heat as a stimulus), a methodological paper has appeared on the subject. Hill, Flanary, Kornetsky & Wikler (34) from previous workers' experience reached the conclusion that power, not voltage, is the critical stimulus factor and that a psychological rather than a physiological "report" is the more significant response factor for studies on man. The voltage necessary to produce the desired amperages or wattages could not be predicted by Ohm's law from knowledge of skin resistance because of the capacitance factor when skin impedance is high. This was solved by constructing curves relating volts to ohms necessary for a constant psychological response. An apparent power, as predicted from voltage with these curves, proved to correlate more closely with psychological response than amperage when skin resistance was changed. Under both low and high skin resistance conditions, the mean error for apparent power was less than half that for amperage ($P < 1$ per cent).

Skouby (54) studied the effects of cholinergic drugs on pain receptors in experiments paralleling his earlier ones on cold receptors. Heat stimulation with a thermode, equipped with a thermocouple, usually gave a threshold variability (i.e., σ) of 0.1°C . A stable acetylcholine-like substance passed into the skin by iontophoresis lowered the pain threshold markedly (max. 3.5° to 5.5°C .), but the dosage is indeterminable in such experiments. Intracutaneous injection of it in doses of 1 to 40 μg . and of acetylcholine in doses of 0.1 to 1 μg . decreased the pain threshold significantly. The acetylcholine potentiator, prostigmine, also decreased the pain threshold, indicating that the amounts of cholinergic substance present or released in the skin was sufficient to affect end organs provided the enzyme systems were poisoned. Atropine in dosages of 50 μg ., intracutaneously, raised the pain threshold, but only

after the remarkably long latency of 10 to 30 min. and in a degree barely significant. Unfortunately, the atropine experiments were not sufficiently definite to conclude that the concentrations of cholinergic substances existing in the skin are sufficient to lower the threshold. The author argues that the concomitant flushing and sweating are not regularly, and hence not causally, related to threshold changes. These experiments raise interesting possibilities in certain clinical pain syndromes—e.g., herpes zoster.

Jalavisto, Orma & Tawast (36) used a jet of air interrupted by slotted disks to obtain a corneal stimulus of controllable duration and intensity. The resultant strength-duration curves for just perceptible sensation were hyperbolic, like those for stimulation of nerve with electrical current. Sensibility decreased with age, detectably during the first 50 years and clearly thereafter. The quality of the minimum perceptible sensation was described as touch rather than pain. The authors pointed out that the sensory loss (with age) could be due to either decreased neural activity or increased corneal rigidity resulting in less deformation for an equivalent stimulus.

Gregg (23) lists the likely instrumental errors in electrically driven apparatuses for testing vibratory sensibility as follows: (a) differences in the acoustical impedance or "stiffness" offered by various test areas alter the amplitude by as much as 20 per cent, (b) warm-up and aging of the mechanism may cause errors in excess of 10 per cent, and (c) pressure and direction of the application to the test area. Gregg's apparatus incorporates an amplitude-measuring device into the vibrator head and the apparatus was calibrated optically. In 1200 observations on 100 subjects the deviations of each subject from his own threshold yielded a normal curve with a 3 per cent probable error. A table is presented showing the threshold amplitude for various body areas for one subject.

SOMESTHESIS: PERIPHERAL NERVE AND ROOTS

Sinclair & Hinshaw (52) examined the sensory changes in cold block in nerve by the simple expedient of immersing the elbow in ice water, a dynamic experiment for the experimental psychology laboratory. Except in time course, cold block resembles procaine block. At first there is a mildly unpleasant numbness. Touches seem dull and "far away," then slightly delayed in onset and prolonged in duration, and finally, the threshold increases and touches are "missed." The changes in pain during cold and procaine block are similar, and in neither is there marked over-reaction. Thermoreceptive phenomena are marked by intensification, delay in the form of a slowly developing sensation, or a long delay followed by a sudden, explosive development of the sensation. The whole matter of conduction through nerve blocks is proving complex and incapable of any simple explanation such as fiber size.

In a study of 31 normal subjects (53) these authors compared in detail the temporal course of sensory changes resulting from three types of experimental nerve blocks—procaine, compression, and cold. The variations

between the three types of block do not lend themselves to brief summary.

Cronholm (16) has contributed a monograph which indicates that the phantom limb phenomenon is a complex central phenomenon and one of many interesting sensory phenomena which could be studied to profit, if experimental psychologists had ready access to neurological patients.

In passing through the posterior root ganglion, sensory impulses are delayed over and above conduction time by 0.08, 0.14, and 0.23 msec. in alpha, beta, and gamma fibers, respectively. Dun (18) ascribes this delay to the bifurcation of fibers, which also renders conduction liable to block and to interaction between neighboring fibers. Whether such phenomena at the ganglion are involved in sensory phenomena is not known.

SPINAL PATHWAYS FOR SOMESTHESIS

In an experimental degeneration study of the spinothalamic tract on 15 monkeys and 5 cats, Morin, Schwartz & O'Leary (46) stress the fact that anterolateral cordotomy interrupts many ascending fibers besides those destined to enter the thalamus and that more information about the brain stem course of these components is needed to understand how the diffuse thalamocortical systems are activated. At the spinal cord level, these authors confirmed that the caudal contributions to this system shift toward the periphery of the tract, but it is the degree of posterior shift which is really the controversial question. One component was traced from the ventrolateral, white columns into the reticular substance of the medulla from where after synapsing once or more it probably continues on to the thalamus and cortex. The existence of a contribution to Gudden's commissure, described by Chang & Ruch, was also confirmed, but no further light was shed on its functional role.

By studying ascending degenerations in man, Gleses & Bailey (20) transferred Walker's general findings on lamination of the spinothalamic tract to the human level. They also confirmed that many fibers disappear from the ventrolateral tract before it reaches the thalamus. Therefore, to study the spinothalamic tract itself, the level of the superior colliculus was chosen. They counted and measured the fibers in the region occupied by the spinothalamic tract as seen in Weigert preparations. There were only 1500 such fibers. All had small diameters (mostly 4 to 6 μ) corresponding to a calculated conduction velocity of 25 to 35 m./sec.

The location of the contributions from the sacral segments along the posteroventral dimension of the spinal cord is particularly interesting because of the heavy influx of visceral afferent fibers (bladder and bowel) at this level. Theoretically, it is of interest to know whether visceral fibers form a distinct viscerosensory pathway or whether they regroup to travel with somatic fibers of like kind—e.g., pain components in the spinothalamic tract or stretch afferents in the posterior or posterolateral columns. This information bears on the question whether functional or topographical considerations determine the course of sensory pathways. Gleses & Bailey

placed the sacral contributions to the anterolateral columns more ventral than in Walker's schema, suggesting that regrouping may not occur. Nathan & Smith (48), in an extensive study based on 27 cordotomy patients with 18 histological controls, concluded that the somatosensory impulses underlying the desire to micturate traverse fibers lying opposite the dorsolateral process of the anterior horn, i.e., in the same region of the cord as those conveying impulses interpreted as pain, warmth, and cold from the sacral segments of the body. Previous work by Barrington has indicated a more posterior position in the lateral columns for the afferents serving awareness of bladder fullness than for those serving bladder pain, which better explains the escape of bladder sensation in cordotomy. This problem is one which may well require behavior techniques for solution.

SOMESTHESIS AND THE CEREBRAL CORTEX

Cortical responses in general.—Prior to World War II, the development of the evoked potential technique by Marshall, Woolsey & Bard placed in the hands of physiologists a powerful tool for investigating the cortical processes underlying perception, especially localization and two-point discrimination. The reviewer has discussed some of the implications of the recent studies employing this method (51). The cortical projections of virtually all channels for special and common sensibility have now been carefully plotted. The somatosensory systems preserve the dermatomal arrangement which, in turn, is a rather crude topographical organization. In the spinal cord, there is a mixture of the functional and topographical in the patterning of the ascending tracts. Grouping of pain fibers from three depths—cutaneous, muscular, and visceral—is an example of functional organization. Lamination of tracts is an evidence of topographical organization. In the thalamus and cortex, topographical organization takes precedence over functional (modality) localization. Representation of the body surface consumes the available dimensions on the plane surface which is the sensory cortex, so that depth as well as modality is merged into a single isomorphic scheme. New evidence that visceral and muscular impulses project according to the dermatomal pattern is described in detail below.

The degree of point-to-point representation of the periphery in the sensory cortex is emphasized by the use of prolonged anesthesia sufficient to depress the spontaneous cortical rhythm. Point to point-of-maximal-response is a more accurate description. This maximum is certainly not the result of fixed 1:1 neuron chains, but represents a dynamic statistical process since cross connections exist at each level. The degree of convergence and divergence is so great that its functional advantage should come to light if the afferent systems are studied from a functional rather than a "functional anatomy" approach. Such studies have been made during the year.

Interaction.—The cortical potential evoked by nerve stimulation can be studied with the conditioning-testing shock technique traditionally used to analyze such axon and synaptic properties as refractoriness, summation, and

inhibition. The two shocks are applied to different nerves or nerve branches. The modifications made in the cortical response to the "test nerve" stimulation by a preceding or simultaneous stimulation of the "conditioning nerve" denote interaction, the subvarieties of which have been intensively analyzed by Amassian (5). The generic term "blocking interaction" is applied to any reduction in the test response induced by the conditioning stimulus and the term is especially useful when the circumstances do not permit more explicit identification of the mechanism. When the cortical response is reduced by stimulating a second nerve with a conditioning shock which does not activate any significant number of elements in the region of the recording electrode, the phenomenon is termed "inhibitory interaction." A deficit in the test response caused by elements remaining in an unresponsive phase after the conditioning activity is termed "occlusion," provided the elements are activated by both conditioning and testing shocks separately.

In view of the characteristic brief latency of the evoked potential which denotes a rapidly acting system, the temporal course of inhibitory interaction is surprisingly long, 100 msec. for complete recovery of the primary response and 180 msec. for recovery of the negative wave. The positive wave of an evoked cortical potential is now believed to represent invasion of the cortical neurons in layer IV; the following negative wave to represent spread to other cortical layers.

The evoked cortical potential is an end product of the chain of reactions which begins at the nerve stimulated. The interaction manifested at the cortex obviously might result from interaction at one or more subcortical levels. Inhibitory interaction was not observed in records from the medial lemniscus although some blocking interaction between closely related (spatially) afferents occurred as first described by Therman. Recordings from the subcortical white matter revealed inhibitory and occlusive interaction which therefore must occur at the thalamic level. However, thalamic interaction alone probably does not account for all the inhibition and other blocking deduced from the changes in the cortical primary response. Blocking, at least, can occur at a cortical level; a cortical stimulus exciting the cortical U fibers can block the response to a peripheral stimulus in an adjacent cortical area.

The spatial aspects of sensory interaction are, at first sight, surprising. In respect to depth, it is manifested between all combinations of cutaneous, muscular, and splanchnic afferent fibers. This is understandable on a morphological basis because of the convergence of superficial and deep afferents. More surprising, in view of the topographical organization of the ascending somatosensory systems, is the fact that impulses coming from widely separated areas of the body surface can interact.

These studies suggest that the neural mechanisms of spatial discrimination in the somatosensory system will eventually prove to involve interaction, especially inhibitory interaction, in some as yet unspecified manner. But if interaction cuts across the topographical organization of sensory

systems (isomorphism), then some aspect of interaction, perhaps the temporal pattern of impulses, must be called upon to explain perceptual functions traditionally linked with place.

Visceral sensation.—Conspicuously neglected among the senses are those served by impulses conducted over visceral nerves. Amassian, using the evoked potential technique, has discovered that splanchnic nerve impulses are projected to the cerebral cortex (4), and has analyzed this input from the point of view of fiber group and spinal pathway (3). In fact, three papers (1, 4, 17) on this topic, from independent workers, have appeared during the year. They confirm and complement one another to a remarkable degree.

A splanchnic representation was found in both sensory areas I and II in the cat and dog, but only in sensory area II in the rabbit, and sensory area I in the monkey,—an interesting phylogenetic difference. This and the fact that the second sensory area has developed in proximity to the oldest part of the cerebral cortex suggest the possibility that sensory area II is more primitive than area I although the results could merely indicate a difference in anesthetic susceptibility. The splanchnic afferents were bilaterally represented in somatosensory area II but not in area I, although in area I some ipsilateral responses were observed in the monkey under very light anesthesia.

For all areas and all animals, the evoked potentials were maximal in the cortical area lying between the arm and leg representation—i.e., coinciding with the trunk representation determined by tactile stimulation in the same animals. This is the position which was predicted from the segmental level at which the splanchnic afferent fibers enter the spinal cord. If functional rather than topographical principles determine cortical representation, this splanchnic representation should coincide with vagal and pelvic nerve representations. Altogether, the observation indicates the predictive value of the topographic principle in guiding the search for unknown localizations.

The fact that the splanchnic nerve projects to the topographically-organized postcentral gyrus is of itself highly significant. It suggests that these particular splanchnic impulses have the requisite central mechanism for "local sign" and spatial discrimination. In contrast, no evidence that the vagal nerve projects to the postcentral gyrus was obtained (51), from which it may be deduced that any vagal representation found elsewhere has a different significance. Apropos of this study, it should be pointed out that the postcentral projection of the splanchnic nerve revealed by these three studies is the only visceral cortical representation which has been unequivocally demonstrated.

Where nerve, rather than sense organ stimulation, is employed in evoked potential studies, it is of utmost importance to identify the responsible fibers in an effort to establish the functional (modality) significance of the cortical localization. The following statements, based on Amassian's analysis, apply to the initial surface-positive cortical deflection. The responsible fibers had a low threshold and therefore were presumably of large diameter. This was

confirmed when their rapid conduction velocity (maximum 64 m./sec.) was determined directly. Since many of the large afferents are derived from pacinian corpuscles, and since mechanical stimulation of the viscera evokes the cortical response, the main cortical deflection probably originates in part from pacinian corpuscles.

When the splanchnic nerve was stimulated more strongly, additional deflections appeared in the nerve potential. These correlated with deflections appearing on the returning limb of the primary cortical response. At the same stimulus strength, there was a multineuron type of reflex discharge from the intercostal nerves which did not appear with the weaker stimuli that yielded the large initial cortical response. It is unfortunate that this group of fibers cannot be studied in isolation as they, quite probably, serve visceral pain.

Section and microelectrode recording from the posterior columns showed that the fast splanchnic impulses were conducted in the posterior columns along with deep somatic impulses. This relationship is a further point in the similarity between visceral and deep somatic afferent systems stressed by Lewis. The later, high-threshold component of the initial response depended on impulses conducted in both the posterior and anterolateral columns.

Further indications of the topographical (dermatomal) organization of sensory systems are that the ascending fibers conducting the fast impulses from the splanchnic nerve occupy a position in the posterior column between those conducting impulses from the arm and leg, and that this relationship is maintained in their thalamic terminations. Aidar, Geohagan & Ungewitter (1) traced the pathway with microelectrodes through the ipsilateral gracile fasciculus and nucleus, and in the medial lemniscus from its decussation to the posteroventral nucleus of the thalamus. At the latter level, the position between leg and arm relay was proved by Patton & Amassian (49).

Finally, as Amassian points out, the discovery of the splanchnic cortical projection dictates that the postcentral gyrus be termed the somatovisceral sensory area rather than simply the somatosensory area.

Deep sensation.—In contrast to the large fiber components of the splanchnic nerve with their strong cortical projection, the largest fiber groups in the mixed spinal nerve according to Mountcastle, Covian & Harrison (47) do not project to the cerebral cortex. Such fibers (13 to 20 μ in diameter), designated group I fibers by Lloyd who analyzed their reflex connection, are derived solely from muscle branches; they give rise to monosynaptic myotatic reflexes and, according to Lloyd & McIntyre, disappear quickly from the posterior columns to enter the spinocerebellar tract. Mountcastle *et al.* showed that the weak stimulation of a muscle nerve at Group I strength, which elicits a maximal monosynaptic reflex discharge, does not evoke a cortical potential. When the nerve was stimulated with progressively stronger shocks, the cortical response appeared only when the shocks considerably exceeded multisynaptic reflex strength and caused a late multisynaptic re-

flex discharge. The latency of this cortical potential was 18 to 20 msec., nearly twice the latency of a cortical response evoked by stimulating a mixed spinal nerve or the splanchnic nerve. The long latency was not due to relay through the cerebellum. It is the "impulses in the high threshold, slowly conducting, myelinated afferents of muscle nerves, which elicit polysynaptic reflexes at the local level [which] are relayed to the cerebral cortex" (47, p. 346). Since the afferent neurogram was not monitored and since the upper limit of Group III fibers is placed rather high (8μ) in the fiber spectrum, the identification of the cortically projected impulses with Group III fibers and especially the implication that the cortical projection is nociceptive are somewhat doubtful.

Muscle stretch did not evoke a cortical potential when mechanical spread to other tissues was avoided, thus disproving the cruder observations of Gay & Gellhorn. However, light mechanical stimulation of receptors in the periosteum, interosseus membrane, joint capsule, etc., evoked a cortical potential with a latency, thalamic relay, and cortical distribution similar to those for touch. Gardner & Noer (19) also found a marked contrast between the ease with which cortical potentials are evoked by stimulating nerves with cutaneous, joint, or interosseus membrane components as compared with branches to muscles. After antidromic stimulation and partial cord sections, these authors concluded that few impulses from muscle nerves traverse the posterior columns.

These remarkable findings raise many interesting problems. Do the true muscle sensory impulses conducted in large fibers pass through the cerebellum before reaching the cerebral cortex? Or is muscle sense actually fascia, tendon, and joint sense? Until this paradox is resolved, perhaps the clinical term "deep sensibility" is safer than "muscle sense." Mountcastle *et al.* (47) point out that, since the deep receptors are so exquisitely sensitive, it is unlikely that mechanical stimulation of a limb activates only tactile receptors. Also, do the impulses underlying such proprioceptive discriminations as weight lifting traverse other than the posterior columns? Unpublished behavior experiments indicate that they do.

Mountcastle *et al.*, working on the cat, found that deep nerve stimulation or a light mechanical stimulus evoked potentials in somatic sensory areas I and II of the contralateral cortex and somatic sensory area II of the ipsilateral hemisphere, but never in the cat's motor area. Deep nerve stimulation in the monkey evoked typical, surface-positive potentials in the precentral gyrus and in the parieto-occipital association region (51). The potentials in the latter area were of long latency; whereas those recorded from the precentral gyrus had a latency as short as any recorded from the postcentral gyrus in the same experiment. (Not all points in the latter gyrus—e.g., those buried in the central fissure—were recorded.) The precentral potentials were more fragile than the postcentral potentials and were not recorded after ablation of the postcentral gyrus. However, the latencies

strongly suggest that impulses from deep nerves reach the precentral gyrus without traversing the postcentral gyrus.

By strychnine neuronography, Amador, Shimizu & de Brux (2) demonstrated that the efferent projections of the inferior part of the parietal association area are mainly confined to the surrounding areas although more distant projections were found in restricted areas. Virtually the whole cerebral cortex has now been studied by neuronography, but the functional anatomical information it has provided has far outstripped functional knowledge of the fiber connections revealed.

Behavior studies.—In the period covered by this review, there have been very few behavior studies on the somesthetic area. Chow, Blum & Blum (14) have reported experimental ablations of the parieto-temporal occipital region in monkeys involving more extensive lesions and a wider variety of somesthetic discriminations than were used in the earlier studies by the reviewer. They also studied the effects of adding prefrontal lesions to the more posterior ones. Consistent with the earlier results, some of the somesthetic discriminations tested presented initial postoperative difficulty, and in threshold series no final threshold difference was demonstrable. The amount of relearning required varied considerably from test to test, as did the effects of subsequent prefrontal lesions, with some tendency for a reciprocal effect on a given test. The results do not lend themselves to brief summary. However, the authors concluded that the

neural substrate which is critical for retention or ready reacquisition of certain habits is organized in discrete centers specific to the functional categories. These areas of concentration are supplemented by overlapping fringes of secondary significance.

Zubeck (61) has re-examined somesthetic localization in the rat's cortex in light of the discovery of the second somatic sensory area. A lever-depressing apparatus, employing wire mesh of unspecified size as the stimulus and permitting quick change of stimuli from side to side, was used to test roughness; no threshold determinations were attempted. Neither lesions in the motor cortex nor complete decortication, sparing the somatosensory areas, significantly affected discrimination. In five animals combined lesions of sensory areas I and II markedly impaired performance, and more trials were required for relearning than were needed for original learning. The effects of separate ablations of sensory areas I and II, considered with those of the combined lesions, indicate that area II is less important to roughness discrimination than area I. The fact that the rats performed at chance level for 7 to 10 days following sensory area I lesions contrasts with previous primate studies using emery paper and extensive preoperative threshold discriminations. In the latter, the performance was well above chance level on even the first postoperative tests.

Evaluating these experiments and relating them to previous studies is difficult because no threshold determinations were made. It is impossible to

know how far above threshold the discriminatory capacity was tested in the postoperative series. Also, when two or more preoperative thresholds are obtained, rather than trials to meet a criterion, thorough establishment of the habit is insured. Bar pressing with a shorter time lapse between receiving the stimulus and making the final choice would seem to have little advantage over the modified pull-in technique (emery paper mounted on drums). Judging from those two studies physiologists and psychologists, using essentially similar behavior tests in studying cortical function, seem to place the accent differently, the former tending to be interested in capacity and the latter in the "loss of habit."

Clinical studies.—Bender (8) has continued to study the pathological sensory phenomena which he has termed "sensory extinction" and "sensory displacement." If a patient with disease of the brain or spinal cord was stimulated simultaneously at two points, and one stimulus was perceived and correctly localized while the second was perceived but mislocalized, "displacement" was said to have occurred. (If one was not appreciated at all, "extinction" had occurred.) The mislocation was not haphazard but tended to have consistency of direction (hence "displacement")—i.e., to the same side, to the opposite side, or to extrapersonal space. This disorder of localization and projection of sensation followed certain rules. When the right side of the face and the left hand were touched, the sensation tended to be displaced upward from the left hand to the left side of the face. The face, therefore, is said to be "dominant." Contralateral displacement was usually away from a region of diminished sensibility. Extinction, obscuration, and displacement are seen not only in neurological patients but also in normal adults and more frequently in children (9). Bender points out that the sensation evoked by one stimulus obviously influenced the response to the other, sometimes extinguishing or obscuring, sometimes causing it to be wrongly localized. By way of interpretation, Bender states:

When a sensory area is stimulated, the impulses enter into a nervous system which is in a constant state of organized activity. The locus of a sensation is thus determined by a background which, although not static, is stable and patterned in its inner relationships.

To some extent this dynamism is a gratuitous assumption, at least for extinction. As pointed out above, recent studies by Amassian have shown that sensory impulses from widely separated areas of the body interact negatively at the thalamus and the cortex. Time relations in this process are critical and should be examined in patients.

Cohn (15) emphasizes rostral dominance in the phenomenon of sensory extinction. With stimuli applied simultaneously to the face and hand, the latter is extinguished, and stimulating the hand extinguishes stimulation to the foot, and a proximal stimulation extinguishes a more distal one. If in fact these are the rules which extinction obeys, they can be explained. Unless compensated by larger fiber diameter and faster conduction rate, the

shorter conduction distance for cephalad and proximal regions would supply the necessary condition for blocking interaction, namely time precedence.

In a series of experiments, related more to the control of movement than to sensation, Chernikoff & Taylor (13) studied the reaction time to the kinesthetic stimulation from a sudden arm displacement. They wanted to learn whether kinesthetic return and the reaction to it are rapid enough to serve as a feed-back to control voluntary movement. The arm could be stopped in 129 msec., a key pressed by the other hand in 149, and an auditory and a tactual stimulus responded to in 151 and 160 msec., respectively. This study is one more indication that servomechanism engineers' feed-back is allegorical and not an actual neural mechanism for the control of movement.

The effect of chronic pain conditions on pain sensibility was examined by Kennard (41). No significant difference was found in the threshold for pain (electrical stimulation with the Grass apparatus), in the reaction to pain (galvanic skin response), and in trials necessary to condition a pain response to an auditory stimulus. Consistently with the high level of anxiety expected in the patient group, the conditioning time was short, but was equally short in a control group of nursing and medical students.

The discrepant results to date on the analgesic action of morphine are ascribed by Hill *et al.* (35) not to the defects in the radiant heat method but to the influence of emotionally-tinged anticipatory responses. They were interested in the possibility that morphine acts on the latter rather than on the actual pain threshold. The action of morphine on pain intensity discrimination using electrical shock as a stimulus (34) was tested in an "anxiety" and an "anxiety-allayed" situation. The anxiety was merely that attendant to the experiment, and its effect was to cause the electric shock to be overestimated during the first three series. This effect was significantly reduced by administration of morphine, but not of placebos. Neither morphine nor placebos had a significant effect on pain threshold in the anxiety-allayed situation. The authors concluded that morphine acts by eliminating the anxiety associated with the anticipation of pain and that such anxiety is an important variable which must be controlled in experimental investigation of problems related to pain.

The interference with pain sensibility resulting from small traumatic injuries to the cerebral cortex was demonstrated by Marshall (45) in a series of 18 cases. The testing techniques were substantially those used by Head. In 11 cases clear impairment of pain and temperature existed several years after small cortical injuries. Information on depth of injury, usually available from operative notes, was sufficient to exclude the possibility of direct damage to the thalamus. It appears from this and a previous study by Russel that extensive cortical lesions which severely impair two-point threshold and position sensibility may leave pain perception little affected; whereas smaller lesions do impair pain sensibility. The writer offers a speculative interpretation of this.

OLFACTION AND TASTE

Kelemen (40) furnishes a thorough account of the nasal cavity of the guinea pig. The anatomical structures are shown in a series of photographs and are interpreted and labelled in line drawings beneath each photograph. He believes that the guinea pig is preferable to the rat as an experimental animal for olfactory work because

The roominess of the nasal cavity, with absence of accessory sinuses, results in less pathological change and a more intimate contact of liquid or gaseous materials with the mucous membranes of the respiratory and olfactory parts.

Harrison & Harrison (27) have contributed a methodological paper in the psychophysics of taste threshold studies. Ash (6), in reviewing gustation in the infrahuman mammals, provides a convenient tabulation of the experimental results available. He emphasized the gaps in our knowledge of gustation in animals, both in methodology and in the number of species studied. He stresses, as do those who have used it, *faute de mieux*, that the preference method does not yield absolute thresholds.

In a monograph covering mainly the somatic and autonomic responses to stimulation of rhinencephalic and related mesopallid structures, Kaada (37) reports some work of significance to sensory processes. Original experiments on the distribution of potentials evoked by stimulating the olfactory bulb in cats and monkeys are described, and he also reviews the physiological and anatomical literature on this topic. The olfactory system is peculiarly difficult to study because it has diffuse, widely-ramifying, neuronal connections and because it does not yield the initial surface-positive response which has proved an effective guide to other primary sensory areas in the isocortex. Positivity was replaced (or preceded?) by two negative waves. The reason apparently lies in the tract fibers entering the cortex from the surface rather than the undersurface as they do in other sensory areas. An initial negative wave appeared to be associated with olfactory tract activity. The distribution of the later negative waves, taken to represent invasion of the primary olfactory area, agreed quite well with an earlier physiological study by Fox *et al.*, and with a recent histological study by Meyer & Allison. Only regions in the immediate neighborhood of the olfactory tract and in the rostral piriform cortex (pre-piriform and peri-amygdaloid areas) showed this activity. On the basis of later positive activity, impulses were assumed to spread from this area into various structures surrounding the "primary olfactory cortex." In the monkey, this positivity occurred in postorbital frontal, anterior insular, and temporal polar areas, which have not previously been considered part of the olfactory sphere. No responses were recorded from the hippocampus, the retrosplenial, the posterior part of the hippocampal gyrus, or the anterior cingulate cortex, all of which have been thought to be the primary sensory olfactory areas. Thus, only the most rostral por-

tions of the hippocampal gyrus appear to be closely concerned in olfactory sensation. These results add to the growing belief that the bulk of the rhinencephalon along with the mesopallium, with which it connects, serves a psychomotor function, specifically emotional behavior.

However, a study by Berry, Hagamen & Hinsey (10) gives reason to pause before rejecting the classical views of the olfactory system. Since they employed a more depressant anesthetic than Kaada and bipolar rather than unipolar electrodes to reduce the breadth of pick up, their positive findings should be given considerable weight. After single-shock, olfactory-bulb stimuli, they recorded evoked potentials in the hippocampus, limbic cortex, and the septal region, all parts of the traditional olfaction-dominated cortex. The latencies of these potentials were all very great and their area of distribution corresponded well with those anatomically ascribed to neuron chains from the rostral olfactory regions. Parts of the thalamus and basal ganglia yielded potentials and this distribution supports Kappers' theory of an olfactory striatum. The divergent results in these two studies probably hinge on methodology and they may be more apparent than real—e.g., the long latency impulses may not be concerned in sensory processes. The two studies, taken together, indicate that the results gained with such facility by electrical methods still must be functionally validated, which can be achieved only by the laborious behavior techniques.

Goetzl, Ahokas & Goldschmidt (20a) continue to investigate the relationship between food intake and chemoreception in man. They consider that a fall in olfactory threshold between an after-breakfast and before-lunch test and a rise throughout the afternoon are established phenomena and are attempting interpretation in terms of satiety. Their data would indicate the ingestion of sucrose in 20 g./135 cc. solution (15 per cent) is equivalent to "no lunch" in that no increase of threshold follows; whereas a 20 g./100 cc. (20 per cent) sucrose solution does increase the threshold though only about half as much as does a meal. For reasons not stated the results of ingesting stronger solutions or sugar in cube form, though obtained, are not presented. The authors' position from this and a previous experiment is that to have a threshold-elevating effect the sucrose must be ingested and tasted, and must be strongly tasted since the weaker solution was without effect. The comment of last year's reviewer that similar experiments by this group lacked most of the essential controls still holds, and to this criticism can be added, the present paper lacks the necessary data since part of them are withheld and no indication of statistical reliability is given. These defects have in some degree been repaired in the following paper. Such an effect of sugar ingestion, were it established, would rule out such obvious errors as masking through food particles left in the mouth, though not olfactory adaptation or a result of eating.

Hammer (25) introduced one of the essential controls in establishing a specific relation of chemoreceptive thresholds to food intake, namely, the

introduction of a flicker fusion frequency determination. He also added a "high caloric lunch" situation. In all three tests acuity was lower just after breakfast than just before lunch, and after lunch the acuity was low and increased somewhat throughout the afternoon. The results on critical fusion frequency suggest that the effect of food intake is not specific for chemoreception, although the authors do not draw that conclusion. For odor and taste the acuity continued to increase throughout the day if no lunch was given but the visual test showed the early afternoon fall in acuity, whether lunch was given or not. These experiments substantially confirm those of Goetzel & Stone though the acuity tended to be lower in the late afternoon tests than at 11:00 (fatigue effect?). The phenomenon seems to represent a complex mixture of fatigue effects, nonspecific, and specific effects of food intake on chemoreception.

Whether all taste fibers from the anterior two-thirds of the tongue leave the fifth nerve with the chorda tympani to join the facial nerve has been debated for some 130 years and is now generally given a positive answer. Harris (26) offers a convincing explanation, based upon careful and thoughtful observation of clinical patients, for the cause of the divergent results which have kept this controversy alive. His observations suggest that it is the loss of common sensibility after trigeminal neurectomy which interferes with the perception of taste rather than the fifth nerve's actually conducting taste impulses. This interference with taste may last for hours, days, weeks, or even years according to the individual's dependence on fifth nerve sensibility. A critical point is that the loss of taste may also be manifested in the glossopharyngeal field, the posterior two-thirds of the tongue. In developing this essentially psychological explanation, Harris employs Börnstein's hypothesis that the pathways for taste and cutaneous sensibility overlap in the thalamus and cortex. There is considerable experimental evidence that this is true of the thalamus.

Patton & Amassian (50) studied the distribution of potentials evoked in the cat's cortex by stimulating the chorda tympani fibers of the lingual nerve (the trigeminal component having been sectioned centrally). Typical, short latency, surface-positive responses were recorded. These were abolished when the chorda tympani was destroyed central to the point of stimulation. Maximal potentials were recorded from a 7-sq.-mm. area, superior to the rhinal fissure and rostral to the tip of the anterior ectosylvian fissure. Thus it lay where it would be predicted on topographical principles, overlapping the projection zone for tactile impulses from the tongue as determined by the same workers. This region lies well anterior to the insula which, along with the free lateral, mesial, and orbital surfaces of the cortex, was consistently silent. The possibility that the few tactile fibers in the chorda tympani are responsible (and a few fibers are sufficient for a large cortical potential) can be partly discounted because of latency differences, and Patton & Amassian's

failure to find a projection to any other area is presumptive evidence that their area is the cortical taste area. Here, as in the study of the olfactory system, knowledge of the cortical localization of the "lost senses" will probably be gained by the conjoint use of electrical recording and behavior techniques, with the former indicating likely localizations and the latter providing functional validation.

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INDIVIDUAL DIFFERENCES^{1,2}

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The present chapter is concerned with both the methodology and the data of differential psychology. The development of new tests, as well as current trends in the construction and evaluation of psychological tests, will therefore be considered, together with the facts and concepts pertaining to individual and group differences. Boundaries between topics are, of course, arbitrary. Although statistics is an important tool of the differential psychologist, any publications dealing solely with statistical techniques have been excluded from the present survey since they fall properly within the scope of another chapter in this volume. All projective techniques and all purely clinical studies on test validation have been omitted for a similar reason. Other areas covered elsewhere in this book which border upon the content of the present chapter include: child psychology, in so far as it employs a developmental approach; counseling, which shares with differential psychology a concern with the evaluation and use of tests; educational psychology, in which individual differences are, of course, of fundamental importance; personality, especially with reference to the study of individual differences in nonintellectual aspects of behavior; and the study of special disabilities, dealing with a particular aspect of human variability. Reference to these other chapters is recommended for a more comprehensive view of current developments in differential psychology. At the same time, a minimal amount of overlap is probably inevitable for a coordinated treatment of the area covered within the present chapter.

TEST CONSTRUCTION AND EVALUATION

Basic concepts and procedures of test construction.—A theoretical discussion of mental testing and the logic of measurement is to be found in an article by Comrey (23). In his analysis of some of the familiar difficulties encountered when trying to treat mental test scores as "fundamental measurement," the author lays special emphasis upon the lack of any experimental operation corresponding to addition which can be performed with test scores. He concludes that test constructors should devote their major efforts toward developing measurement techniques designed to satisfy the criteria of practi-

¹ This review covers primarily the period extending from May 1, 1951, to May 1, 1952, although significant earlier publications, not covered by the preceding year's review, have also been included.

² The following abbreviations have been used in this chapter: WISC (Wechsler Intelligence Scale for Children); ACE (American Council on Education); MMPI (Minnesota Multiphasic Personality Inventory).

cal validity rather than manipulating test scores in the attempt to meet the criteria of fundamental measurement. He notes that in some instances these two types of criteria may even lead to contradictory procedures.

Weitzenhoffer (134) gives a provocative and detailed analysis of the same general problem. After reviewing the implications of the ordinal and additive scales of classical measurement theory, he introduces the concept of a multiplicative scale. In such a scale, multiplication produces a supra-extensive dimension different from the dimensions of the factors in the product. This type of multiplication is differentiated from multiplication by an integer, which is a property of additive scales and introduces no new dimension. The author contends that, of the three scales discussed, only the multiplicative scale leads to an algebra which is isomorphic with the algebra of real numbers, and hence only this scale can lead to a mathematical representation of physical phenomena. Through their use of dimensional units and analysis, the physical sciences have come close to the ideal of establishing a system of measurement which is isomorphic with the number system and other mathematical systems. Since psychological measurements are made largely in terms of additive or ordinal scales, however, mathematical representation of psychological phenomena is characterized as far too restricted and quasi-impossible. Attempts to find mathematical representations of existing types of psychological measurements should, it is argued, be abandoned until basic psychological concepts are redefined in terms of magnitudes which can be measured on multiplicative scales.

An outstanding contribution to test construction during the current year is the pair of articles on test reliability by Cronbach (25) and Cronbach & Warrington (26). The former gives an integrated treatment of available techniques for measuring split-half reliability and inter-item homogeneity, including the Kuder-Richardson, Guttman, and Loevinger approaches, among others. Writing Kuder-Richardson Formula 20 in a more general form, designated "coefficient alpha," Cronbach shows it to be the mean of all split-half coefficients resulting from different splittings of a test. Alpha is found to be an approximate index of equivalence and, except for very short tests, of the first-factor loading of a test. A measure based upon alpha and the number of items in the test provides an index of inter-item homogeneity.

In the article by Cronbach & Warrington (26), the reliability of speeded tests is considered and a new formula derived for determining degree of speeding. This formula is based in part upon the ratio between the variance in number of test items attempted by each person and the total variance of the test scores. This is essentially the procedure proposed in an earlier paper by Gulliksen (54). However, Cronbach & Warrington introduce two adjustments in their formula, first, to allow for the fact that some of the persons completing all items may finish before time is called, and secondly, to take into account the correctness of performance on the last two items attempted by each person. It is apparent that even when no one has time to complete all items in a test, speed does not necessarily play an appreciable part in

determining individual differences in scores on that test. Under these conditions, split-half reliability coefficients may be applied in place of more time-consuming retest procedures.

Davis (29) gives a critical discussion of item-analysis procedures, covering difficulty and discriminative power of items, as well as the use of such item data for maximizing test validity and homogeneity. A simple index of item validity, based upon the number of correct responses in the upper and lower 27 per cent of the group is presented by Johnson (74), together with a standard error formula and t-table for use with this index. Gulliksen (55) derives formulas showing the amount of change in various item parameters expected from changes in group mean and variance, and develops item parameters which should remain relatively invariant with respect to such group changes. Bryan, Burke & Stewart (13) demonstrate empirically, with six sets of achievement test data, that correcting total scores for guessing may materially affect item validity indexes computed against such total scores as criteria. This effect was negligible for the easier tests, but appreciable for the more difficult tests. A critical survey of various techniques for scaling item difficulty is given by Burt (15), together with a detailed description of one procedure recommended as the most satisfactory.

Two articles concerned with battery construction should also be noted. Horst (66) derives and illustrates formulas for so altering test lengths that, if total length of battery is fixed and the score is the total number of items correct, the correlation of total score with criterion shall be a maximum. This follows two earlier articles (64, 65) dealing with optimal test length for different conditions. Mollenkopf (92) discusses and illustrates certain important points regarding differential prediction of two criteria. Special attention is given to the intercorrelation between the two predicted criterion scores and the difference between the validities of a test for the two criteria.

Evaluative and normative studies of well-known tests.—Among the specific tests discussed in the literature during the current year, the Wechsler Intelligence Scale for Children has received a particularly large share of attention. The computation of MA equivalents of WISC² scores is described by Wechsler (132) in the event that such MA's are needed to meet legal or other practical requirements. Seashore (104) gives the distribution of discrepancies between Verbal and Performance IQ's in the WISC standardization sample of 2200 5- to 15-year-old children. About one-half of the cases show discrepancies of eight points or more, although the distribution of differences is symmetrical and the mean differences are practically zero at all ages. It is emphasized that a considerable portion of these discrepancies may be attributable to errors of measurement, and caution in the interpretation of intra-individual discrepancies is urged. In a breakdown with regard to socioeconomic groupings, all differences were small and statistically insignificant, with the exception of those within the professional and semiprofessional category, where a large and significant excess of children obtained higher Verbal than Performance IQ's. Moreover, the direction of the other differ-

ences supported the hypothesis that higher Performance than Verbal IQ is more likely to occur in rural than in urban groups and in manual labor as compared to white collar groups. It might be noted that the selection of performance tests in the WISC may be such as to minimize differences in the patterning of abilities in the various socioeconomic groups.

Weider, Noller & Schramm (133) have published comparative IQ tables for WISC and Stanford-Binet IQ's, based on 106 5- to 11-year-old children with IQ's from 40 to 140. Several other studies (41, 78, 93, 96, 125) report comparisons between WISC and Stanford-Binet IQ's on specific groups, including both preschool and school ages and ranging from mental defectives to superior children. In most groups, the mean IQ is significantly higher on the Binet than on the WISC, this difference being greater for the brighter and for the younger children. In a mentally retarded group, the reverse difference was found, the WISC yielding a significantly higher mean. These differences cannot be readily explained in terms of the more highly verbal content of the Binet, since the mean differences between Binet and WISC Verbal IQ were even larger than between Binet and WISC Performance IQ. Correlations between Binet and Full Scale WISC IQ range from the .60's to the .90's.

A number of studies have been conducted on the Wechsler-Bellevue, including comparisons of Wechsler IQ's with Army General Classification Test scores (122) and with Stanford-Binet IQ's (2), as well as an investigation of changes in Wechsler scores upon retests (115, 116). Gurvitz (56) discusses some defects of the Wechsler-Bellevue, such as unrepresentativeness of the standardization sample, irregularities in the preparation of normative tables, and inconsistent scoring procedures in the successive editions. In a comprehensive survey of published abbreviated versions of the Wechsler-Bellevue, Herring (61) reports correlations between the complete Wechsler-Bellevue and 21 different short forms containing from two to five subtests.

Studies of the Arthur Performance Scale have raised questions regarding the adequacy of its standardization. In a sample of mental defectives, Revised Form II of this test yielded significantly lower MA's than Form I; the Stencil Design Test and Healy Picture Completion Test II, in particular, appeared to be standardized at too high a level for such subjects (45). In a comparison of the performance of school children on the Arthur Revised Form II and the Stanford-Binet, earlier results (57) were confirmed in so far as a large majority of the children obtained lower Arthur than Binet IQ's, the mean group difference being approximately 10 points in favor of the Binet (88). Studies of the Stanford-Binet include an evaluation of the short administration of this test by the "limen method" previously described by the authors (82, 90), and a qualitative analysis of the types of errors made by children of different ages in defining words on the Stanford-Binet Vocabulary test (37).

In an analysis of the ACE³ Psychological Examination scores of several groups of college freshmen, the Quantitative scores did not consistently

predict achievement in mathematics courses significantly better than the Language scores (9). These results are in essential agreement with those obtained in an earlier investigation with high school students (121). They represent part of a growing body of data which demonstrate the discrepancies between empirical validity and what has often been presumed to be the logical validity of part-scores on several widely used tests.

Studies on interest tests continue to appear at a rapid rate. Strong (119) reports the results of a 20-year retest of an original group of approximately 600 Stanford University students with the Strong Interest Test. Stability of scores was high over this period, close agreement also being found between interest scores and present occupation. Essentially the same results were obtained in a special analysis of the "engineer interest" scores on this scale over a 19-year period (120).

The Kuder Preference Record has likewise been checked for stability with college freshmen over a 15-month period (100) and with high school freshmen over a three-year period (87). Wiener (136) has called attention to a number of contradictions between the a priori "logical" grouping of occupations under the various Kuder scales and the empirical data from the actual testing of the same occupational groups reported in the test manuals. By reference to such empirical data, Wiener has prepared a coding system for 211 occupations, indicating the high and low scales for each. Frandsen (40) proposes a modification of this coding system, which permits finer differentiation of occupational group profiles. In a study on college students (32), high correlations were obtained between interest and experience in the different areas, but this relationship may have been inflated by the fact that the Kuder blank was used, with different instructions, to determine both interest and experience. A novel use of the Kuder was investigated by Steinberg (114), who found that neurotic war veterans average significantly higher than non-neurotics on the musical and literary scales and significantly lower on the mechanical. The extent to which such a relation is specific to neuroticism in a *military* situation would have to be determined before generalizing regarding the use of this test as an index of neuroticism.

Two new articles on the MMPI² may also be mentioned. One is a normative study on several college samples, indicating that both split-half and retest reliabilities of several keys are extremely low and that some adjustment in norms may be required for college students, since mean scores were consistently and significantly above 50 in some components (49). Cottle & Powell (24) demonstrate empirically that chance errors on this test will not produce a "normal" profile, all mean scores obtained through random numbers and dice throws being at or above 70. The reader is referred to other chapters of the present volume for clinical validation studies of a number of personality tests. Note should also be taken of the publication of the revised form of the Study of Values by Allport, Vernon & Lindzey (3).

Investigations of malingering and simulation.—The principal procedures so far developed to investigate the problem of malingering on both personal-

ity and aptitude tests include: (a) the comparison of groups instructed to fake responses in specific ways; (b) the comparison of test scores with such independent measures as ratings or clinical diagnoses, a method which has rarely proved feasible; and (c) the comparison of scores on the same test administered to groups with varying motivation to fake responses, as in the case of subjects tested for job selection and for research purposes (52). The last-mentioned procedure was followed in a study of real-life applicants and employees with the Kuder Preference Record and the Guilford series of inventories for the measurement of personality factors (52). The results suggest that applicants do in fact fake test responses, the extent varying somewhat with the test.

A study of the Minnesota Personality Scale and the Cornell Index by the first-mentioned method corroborated earlier findings on this type of test in so far as significantly more favorable scores were obtained under instructions to appear well adjusted (95). Within the student groups tested, no relation between ability to simulate and ACE scores was found, but women excelled as successful simulators. A similar study of the California Test of Personality suggested that the test as a whole may measure a sort of "culturally acceptable defensiveness," rather than adjustment (81). The fact that subjects can successfully fake personality test responses for specific jobs was illustrated in another study (135), in which markedly different distributions of self-confidence (F1-C) scores on the Bernreuter were obtained by the same group of 73 students when instructed to simulate applicants for the jobs of salesman and librarian, respectively.

By the same general method, malingering on the CVS abbreviated intelligence scale was investigated with groups of naval recruits and college students, who were instructed to "appear stupid" (99). Tentative "malingering keys" were developed through a comparison of item responses of these two groups with the responses of a group of genuine mental defectives.

New tests.—A number of new psychological tests of special interest have been published or described in the literature during the current year. Several are of the type which would formerly have been labeled "intelligence tests," but which are now more often designated as "general classification" or "screening" tests. Despite the sharp swing to the technically preferable practice of measuring separate factors or aptitudes, the global screening test still exhibits strong viability. There is, of course, no fundamental objection to such tests as rough practical devices, provided that they are interpreted in terms of the specific criteria against which they were validated. Current examples include tests designed for a wide variety of subjects, from infants to college men, and from American naval recruits to culturally isolated and illiterate groups. The Northwestern Intelligence Test B, for Infants 13 to 36 weeks old, was released during the current year (47). This follows the earlier publication of Test A (46), covering the 4- to 12-week period. A test for "high grade intelligence," being developed in England (60), was administered to large samples of university students and staff as a means of evaluat-

ing its suitability for such populations (130). A major event in psychological testing was the development of the Selective Service College Qualification Test by Educational Testing Service, and its administration to over 339,000 college men between May and July, 1951 (38).

Three research projects concerned with the development of nonverbal "intelligence" tests to meet specific needs should also be noted in this connection. These include: the preparation of the Navy-Northwestern Matrices Test, an abbreviated, individual intelligence test designed especially for the neuropsychiatric screening of naval recruits (68); the development of a pictorial "Homonyms" test on Jamaican school children (98); and the assembling and standardization of a performance battery for selecting Gurka recruits in Nepal (131). The report of the last-named project also contains good examples of many of the problems encountered in the testing of culturally isolated and illiterate groups.

A new personality scale for the measurement of dominance was constructed by the empirical selection of questionnaire items in terms of two criteria, viz., self-ratings and "peer group nominations" for most and least dominant (51). Levine & Zachert (80) report the development of eight keys for scoring an interest and biographical inventory with reference to different Air Force specialties. MacKinnon (85) describes the program of test development at the Institute of Personality Assessment and Research, University of California. Preliminary data from this research show close correspondence between responses on a figure preference test, a test of preference for paintings, and the individual's "self-percept" as indicated in an adjective checklist. Promising results are also reported for a series of simple perceptual and cognitive tests designed to tap certain aspects of personality.

FACTOR ANALYSIS

Theoretical and methodological discussions.—Cattell (21) has prepared an introductory textbook on the methodology of factor analysis, designed to be more elementary than previously available books on this subject. In an article on current misapplications of factor analysis, with special reference to personality test scores, Guilford (53) discusses such difficulties as spurious correlations resulting from the multiple scoring of items for more than one variable, ipsative scores (as in the Kuder), in which high scores in one variable must be counterbalanced by low scores in others, and the use of factor analysis when the number of variables is insufficient to overdetermine each factor or when distributions are highly skewed, multimodal, or truncated. In a characteristically thorough critique, McNemar (86) summarizes 10 types of improper procedure which he found exemplified in a survey of 73 factor analyses published between 1941 and 1950. Chief among such injudicious practices are the use of small samples and the failure to consider problems related to sampling error, the factorization of unreliable tests, inconsistent logic regarding rotation of axes and naming of factors, and other problems pertaining to choice of variables and interpretation of results.

Discussing the applications of factor analysis to clinical psychology, Cattell (20) outlines various possible "techniques" of factor analysis, involving the correlation of: tests for different persons (R); persons for different tests (Q); the same tests, or other indices, in a single person on different occasions (P); different occasions for the same tests in a single person (O); different occasions for the same persons in a single variable (T); and persons on different occasions in a single variable (S). Obviously the Q, O, and S techniques merely use the transpose of the matrices in the R, P, and T techniques, respectively. Cattell advocates the P technique as the most suitable for clinical data. It should be noted that Cattell, recognizing a source of confusion between the Q technique as defined above and what he terms Stephenson's "subjective Q technique," proposes the designation Q_s for the latter. With this suggestion, Stephenson (117) takes issue, arguing that all of the techniques described by Cattell merely represent alternative ways of analyzing data, whereas his own Q technique, which antedates Cattell's use of the term, involves a basically different type of data. In the Stephenson technique, a list of trait names is to be ranked with reference to their importance in the individual's make-up, or in terms of some other specified intra-individual criterion. Thus in this process intra-individual norms are substituted for group norms.³

Factor analysis of aptitude variables.—French (42) has performed a special service for test constructors and other psychologists interested in the organization of abilities by integrating the results of 69 factorial analyses of aptitude or achievement test scores conducted on adolescent or adult samples. Only studies employing rotation of axes to simple structure, with either orthogonal or oblique frames, are included. Cross-identification of factors isolated in different studies is undertaken whenever possible. The material has been effectively organized for ready reference, being alphabetically arranged with reference to authors, factors, and tests. A total of 59 group factors, plus a few especially suggestive specifics, doublets, or triplets, are discussed. On the basis of this survey, French favors the hypothesis of a relatively large number of frequently overlapping factors, varying greatly in comprehensiveness but all on an equivalent level. He finds no genuine evidence for a hierarchical theory of trait organization, with subfactors and higher order factors.

Such a conclusion is in sharp contrast to that reached by Vernon (129) from a more comprehensive but less formally organized survey of practically all British and American contributions appearing between 1935 and 1949, as well as many outstanding earlier studies. Vernon advocates a hierarchical group factor theory, in which the major portion of test variance is attributable to g, while group factors of decreasing breadth are located at successive levels of the hierarchy. As in his earlier writings, Vernon adheres to

³ It might be added that the letters R, T, and P were used by Burt in his 1941 book (14, p. 177) with different meanings from those given them by Cattell. Thus O and S so far appear to be the only unambiguous letters in Cattell's schema.

an operational definition of factors and emphasizes the fluidity and modifiability of factor patterns. Considerable evidence is assembled to indicate how abilities may become differentiated and reintegrated into fresh patterns as a result of educational or vocational experiences.

New research on the factorial organization of abilities includes Fruchter's (43) analysis of the Airman Classification Test Battery, in conjunction with a number of other well-known aptitude and achievement batteries as well as measures of educational level and socioeconomic status. Studies on reasoning were conducted by Botzum (12) and by Adkins & Lysterly (1). The former was especially concerned with the interrelationships of the reasoning and closure factors identified by earlier investigators. The latter involved primarily the factorial analysis of 66 variables, including a wide variety of reasoning tests, together with reference tests for previously identified non-reasoning factors. Mention may also be made of studies of narrower scope, dealing with the factorial composition of spatial aptitude (91), clerical aptitude (7), word fluency (77), mathematical achievement (8), and memory (73).

Other applications of factor analysis.—Eysenck (35) describes a hierarchical model for the factorial organization of personality, similar to that which he proposed in an earlier discussion of the structure of attitudes and also resembling the general picture drawn by Vernon for abilities (cf. above). The four levels in Eysenck's model include: "specific response level," illustrated by a single score on a test; "habitual response level," corresponding to test scores freed from error of measurement; "trait level," referring to group factors; and "type level," associated with second-order factors. A factor analysis of 42 objective tests of personality, together with 22 reference tests of factors previously identified in batteries of objective personality tests, is reported by Cattell (19). Tyler (126) summarizes studies involving the factorial analysis of the MMPI scales and presents a new analysis of the nine original scales together with six new MMPI scales which have been described in the literature.

Following the general pattern of Cattell's earlier factorial study (18) of the characteristics of different nations, Hofstaetter (63) analyzed 16 cultural parameters of the 48 states, including data on population, education, and crime. Three orthogonal factors were identified. Still another novel application of factor analysis is illustrated by a study (10) of the "professional interests" of psychologists through the Q technique (in Stephenson's sense). The basic data consisted of the ranks given by different psychologists to 15 psychological journals.

CONDITIONS RELATED TO INDIVIDUAL DIFFERENCES

General discussions of heredity and environment.—In an article by Ginsberg (50), the concept of "instinct," as employed in recent writings by psychologists and biologists, is critically evaluated in terms of eight major logical fallacies, viz., fallacies of contingent connotation, *ad hoc* postulation,

reasoning by analogy, reductionism, simplification, misplaced concreteness, proof by inexhaustive elimination, and false dichotomization. Most of the points are well taken and should exert a salutary influence on discussions of heredity and environment. The presentation would have benefited, however, from judicious condensation and clarification. Examples of the type of fallacies described by Ginsberg can be found in Tinbergen's *The Study of Instinct* (124), a book which has so far received at least one favorable review by a psychologist (17) but which will undoubtedly stimulate much controversial discussion. Although presenting a wealth of interesting data on animal behavior and reporting some ingenious experimental procedures, Tinbergen often employs logically questionable concepts and ignores alternative explanations for the given facts.

The general plan of an extensive research project on heredity and social behavior in mammals, and particularly in dogs, is described by Scott & Fuller (103). This project, which has been in progress since 1946 at the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine, is concerned with such problems as individual and breed differences in behavior patterns, behavior development from birth to maturity, social behavior toward other dogs and toward humans, factorial analysis of test scores and other measures, cross-breeding of outstandingly different breeds, and "cross-fostering" to test the effects of maternal environment upon behavior.

McCandless (84) analyzes the concepts of "intelligence" and "environment," pointing out that both are abstract constructs. He then reviews evidence for the role of environmental factors in intellectual development, citing research on social classes, infant behavior, foster children, nursery school attendance, and aclinical or undifferentiated mental deficiency. In reference to the etiology of undifferentiated mental deficiency, he proposes two hypotheses to suggest how the individual's psychological environment may contribute to the development of such a condition. First, the environment in which such individuals are reared may provide inadequate opportunity for acquiring those skills which are subsumed under our term "intelligence," as for example verbal ability. Secondly, such an environment may likewise stimulate the development of habits which are detrimental to "intelligent" behavior, such as the expectation of failure, concrete rather than abstract thinking, and the like.

Attention is also called to the chapter on "Genetic Variability and Human Behavior," prepared by David & Snyder (28), in *Social Psychology at the Crossroads*. Written by two geneticists, this account of the principles of human heredity lays special emphasis upon psychological implications. Similarly, the last three chapters in the most recent revision of Snyder's *Principles of Heredity* (112), which deal with genetics in man, incorporate much psychological material into the discussion and should be of interest to psychologists. Both surveys show an awareness of the limitations and weaknesses inherent in many of the claims regarding the role of heredity in behavioral differences, as well as a recognition of the important contributions of environment.

Effects of specific environmental factors.—Nissen, Chow & Semmes (94) report an experiment in which tactual, kinaesthetic, and manipulative experience was restricted in a young male chimpanzee from the age of one month to 31 months, by encasing the limbs in cardboard cylinders. In comparison with other, normally reared chimpanzees, the experimental animal showed no retardation in visual discrimination or in general postural and locomotor development, although atypical postures adapted to the restriction of the cylinders were noted. Touch localization on arms or hands and tactuo-motor coordination were, however, very deficient subsequent to the removal of the cylinders. Moreover, during a four-month observation period following the removal of the cylinders, no grooming behavior appeared, nor did the pattern of lip movements and sounds which normally accompany grooming.

The opposite experimental approach is illustrated by the latest account of the intellectual development of Vicki, the chimpanzee currently being reared in a human home (59). After the first three years of life spent in such an environment, this animal is approximately at the three-year human norm in such activities as handling toys and mechanical gadgets in the home, social play, and performance on nonlanguage tests of intelligence. On imitation problems, her performance likewise equals that of human children, and markedly excels that of laboratory reared chimpanzees. As already indicated in earlier accounts of this investigation, language remains the one area in which Vicki is seriously deficient, the difficulties encountered being likened to those of human aphasics.

In an extensive study on practice effects in the intelligence test performance of English school children, Peel (97) reports significant improvement on retests administered after approximately one month, the improvement being highest at an IQ level of approximately 120 to 130. Significant gains were also found by Silvey (109) in two-year retests of college students with the ACE and the Nelson-Denny Reading Tests. Such a finding is in line with the results of earlier studies involving the retesting of student groups [cf. (5, p. 283)]. Whether the improvement results from specific practice effects, test sophistication, attendance at college, or from other factors cannot, of course, be unambiguously established from such a study alone.

Two studies have recently been conducted in Europe in the effort to tease out the specific effects of education upon intelligence test scores. From Sweden, Husen (69, 70) reports the results of group intelligence tests administered to 613 20-year-olds upon induction into military service, all of whom had been tested 10 years earlier while in the third grade. Amount of schooling, which varied widely in this sample, correlated .61 with initial test score and .80 with final test score. An especially significant finding was the relation between change in IQ from initial to final test and amount of intervening schooling. Those with only the compulsory seven years of primary school showed an insignificant mean drop of 1.2 points; all other groups improved, their mean gains rising consistently from 2.1 to 11.0 with increasing amount of schooling. De Groot (30) analyzed the mean IQ's

obtained annually by the applicants to an industrial training school in Holland on a Dutch version of the National Intelligence Test. During the war years, the mean IQ dropped significantly, a fact which was tentatively attributed to poor schooling during the period of war and occupation. Evidence in support of this hypothesis is now provided by the mean IQ's for the years 1949 to 1951, which again rose to prewar level, following the restoration of peacetime schooling conditions.

Physical factors in relation to psychological development.—A correlation of .22 between height and intelligence test scores is reported by Husen (71) for a sample of 2257 20-year-old Swedish military inductees; a second similar sample of 4061 cases yielded a correlation of .20. In so far as these correlations were obtained with adult subjects, they cannot be attributed to developmental rate. The author also discards the hypothesis that the correlation results from socioeconomic status, since correlations obtained within samples from large towns, small towns, municipalities, and rural districts did not differ appreciably from those in the total samples. However, no analysis in terms of occupational level and other socioeconomic variables is provided. Weight yielded a much lower correlation with intelligence test scores, averaging .09 in a number of large samples. A tendency was also noted for the frequency of physical defects to decrease with increasing intellectual level.

To check the theories of Sheldon & Stevens (105), Hanley (58) compared the somatotypes of a group of 18-year-old boys with their junior high school "reputation test" records, obtained four or five years earlier. A few significant correlations were found, the highest being between mesomorphy and traits which fit the stereotype of the "all-American boy." The fact that the social stereotype may have influenced the correlation between somatotype and trait ratings by classmates is recognized by the author.

Hofstaetter (62) presents the hypothesis that speed of maturation is inversely related to the cephalization coefficient of a species, the latter being an index of brain weight in relation to body weight. This relation is clearly demonstrated with data on the chimpanzee and the human infant, although some available data on the behavior development of rats, cats, and dogs are also shown to fit the hypothesis roughly. Hofstaetter suggests that the slower maturing species, which consequently require longer periods of parental protection, may thereby enjoy more opportunities for learning and hence make more effective adjustments to their environment.

Age trends.—A valuable source of developmental data is the graphic atlas on the adolescent period prepared by Shuttleworth (107) from a large number of published sources. Although covering principally ages 10 to 20, the majority of the charts extend to earlier and later ages. Many aspects of development are covered, including physical and health factors, intelligence, special aptitudes, interests, attitudes, behavior maladjustments, education, and occupational and marital data. A supplementary pictorial atlas, containing 52 photographs, is available in a separate monograph (108).

Martin (89) studied the development of number concepts in three- to seven-year-olds, using records of relatively spontaneous quantitative expressions, as well as more highly structured numerical tests. A longitudinal study of the development of number concepts and arithmetic abilities in young children is reported by Ilg & Ames (72). On this basis, a normative scale is presented, showing what the child can do with numbers from ages one to nine. Writing behavior has been similarly surveyed for ages three to nine by the same investigators (4). Lehman (79), in his latest article on age and creative achievement, classified creative workers from 19 fields on the basis of age at the time of death. Since he found that all did their best work in their 30's, regardless of age at death, he concludes that increasing the life span will alter the total output but not the age of maximum creativity.

STUDIES ON EXTREME DEVIATES

Feeble-minded.—Significant differences in test performance between brain-injured and "familial" type mental defectives have been reported in three new studies employing the Bender Gestalt Test (11), the Lowenfeld Mosaic Test (106), and a "picture-object" concept formation test (31). In a study of the responses of mentally defective, borderline, and dull normal adults on the similarities subtest of the Wechsler-Bellevue, qualitative differences in type of response were found between the groups (113). The administration of the Purdue Pegboard to 175 14- to 18-year-old males, with IQ's from 42 to 82, showed them to be significantly inferior to normal samples (16). Although overlapping of normal and defective samples was noted, the defectives as a group proved to be inferior in this measure of manipulative dexterity, even when only cases with IQ's between 70 and 82 were included.

Mental growth curves for untreated institutionalized mongoloid patients are reported by Durling & Benda (33), on the basis of retests with the Stanford-Binet. Kirk (75) describes a study in progress on the special training of mentally retarded preschool children (IQ's of 45 to 80), in both an institutional and a community setting. A systematic survey of problems and procedures in the education of mentally retarded children is given in a recently published text by Kirk & Johnson (76).

Intellectually gifted.—The growing interest in the characteristics and background of intellectually gifted adults is reflected in four recent publications. Roe (101) reports a test-and-interview study of 22 eminent American physicists, which parallels her earlier investigation of biologists. Coming largely from professional homes in which education was emphasized, most of these individuals gave early evidence of their intellectual giftedness. As in the earlier study on biologists, Roe stresses the important role of strong drive, sustained effort, and clear-cut channeling of interests in the careers of these men. In another study, Roe (102) classified the predominant imagery type used in thinking by eminent research scientists in different fields, the data having been gathered in the course of personal interviews. A mailed

questionnaire on background characteristics, work habits, and attitudes was administered by Van Zelst & Kerr (127, 128) to scientific and technical personnel engaged in teaching or research. High scientific productivity tended to be associated with technical competence plus an individualistic type of personality and a dislike for regimentation.

In an effort to study the contributions of motivational factors to differences in the achievement of gifted children, Ausubel (6) conducted an experiment on 79 sixth-grade children with IQ's of 130 or higher. The experimental procedure involved essentially the measurement of test performance under "prestige motivation" and under "intrinsic motivation." Those children with relatively high "prestige motivation" were then compared with a low "prestige motivation" group in such variables as educational achievement, occupational aspirations, extracurricular activities, and background factors. The results were largely inconclusive, probably because of the unforeseen complexity of the motivational situation and the inadequacies of the indices of achievement. A valuable contribution to the understanding of intellectually superior children was the publication of *The Gifted Child* (139), under the sponsorship of the American Association for Gifted Children. Written jointly by many specialists, this book brings together available information on the characteristics, problems, and education of gifted children; an extensive annotated bibliography is included.

GROUP DIFFERENCES

Sex differences.—Two new studies by Witkin and his co-workers have been reported during the current year, both of which include further data on sex differences in perceptual organization. In one of these investigations (137), the subject was required to straighten either the room or the chair, after he had been rotated and brought to rest in the tilting chair. With eyes closed and darkened room, no sex difference in error of adjusting the chair was found. When conflicting visual and postural cues were provided, however, a significant tendency was observed for the women to rely more than the men upon the visual field. In another study (138), the subject was required to locate the direction of a sound under conditions of conflicting visual and auditory cues. The results again suggested that women tend to be influenced more strongly than men by visual cues.

In the previously cited study by Ilg & Ames (72) on the development of numerical ability in young children, a sex difference in favor of boys is reported for both preschool and school levels. In the similar study by Martin (89), a more complicated picture of sex differences is found, the direction of the difference varying with the type of measure and with the socioeconomic level of the subjects. Since the samples were small, such findings are suggestive rather than conclusive, but they do point up the need for reporting sex differences in terms of specific tests rather than over-all abilities, as well as the importance of considering other interlocking variables, such as socioeconomic level.

Socioeconomic level.—The first detailed, book-length report (34) has been published on the Chicago study of cultural differentials in intelligence test items, initiated in 1945 by Allison Davis. The basic data are the responses of groups of 9-, 10-, 13-, and 14-year-old children from contrasting socioeconomic levels to the items in seven widely used intelligence tests. Correlations between total scores and an index of social status varied from .20 to .43 for different tests. Significant status differences at the .01 level were found for about 50 per cent of the items among 9- and 10-year-olds and for about 85 per cent of the items among 13- and 14-year-olds.

Earlier abridged reports of this investigation have already aroused considerable controversy; the more detailed account will undoubtedly stimulate further critical comment, as illustrated by Darley's recent review (27). Perhaps the principal weakness in the approach of these investigators is their inadequate recognition of the extent to which the cultural differences in information, motivation, and work habits manifested in test performance also influence the individual's over-all intellectual development. Removing culturally-biased items from a test does not eliminate cultural differences in behavior. The criteria against which the tests are validated are themselves culturally loaded, and "intelligence tests" are operationally meaningless unless defined in terms of such criteria. To be sure, it is important to investigate behavior differences between cultural groups. But such differences cannot be studied by eliminating culturally loaded items. They can be explored only by constructing tests which sample the behavior functions fostered by each culture.

In an analysis of test results from communities in every state, Thorndike (123) found that community variables correlate higher with intelligence tests than with achievement tests. Among the possible reasons suggested for this discrepancy is the fact that education is relatively standardized in America and it is primarily in nonschool factors that communities differ. Hence educational achievement would be less responsive to community variables than would more general indices such as intelligence test scores. In a preliminary report of a continuing study by the interview method, Maas (83) questions the alleged "psychological freedom" of the lower class child. His evidence suggests that the "core culture" child (i.e., lower-middle and upper-lower class), although physically more restricted and more rigidly reared with regard to weaning, toilet training, and other infant-rearing practices, nevertheless enjoys more freedom of communication with both parents and experiences less fear of his parents. Sims (110) has developed a measure of the individual's social class identification based on a list of 42 widely ranging occupations. For each occupation, the subject indicates whether he believes that people in that occupation generally belong to the same social class as himself and his family, or to a higher or lower social class.

Ethnic groups.—Gilliland (48) reports no significant differences in the performance of Negro and white infants tested in the Chicago area with the

Northwestern Infant Intelligence Test, although on the same test institutionally reared infants scored significantly lower than infants of the same socioeconomic level reared at home. Both findings are in line with trends suggested by earlier studies. In ethnic comparisons, little or no group difference is usually found at the infant and preschool levels, while older children show an increasing divergence with age.

An analysis of the Army General Classification Test scores of slightly over 2000 white and 2000 Negro Air Force recruits in World War II is reported by Fulk & Harrell (44). Significant differences in favor of the whites were found within each educational subgroup, as well as in the total sample. The differences were somewhat smaller and the overlapping more extensive in the upper educational groups, probably because of selective factors. That the group differences in test performance persist even when amount of education is equated can, of course, be attributed to many factors, chief among which are differences in the quality of schooling available to the two groups, environmental differences outside the school, and unequal motivation for intellectual development.

A number of recent studies have been concerned, not with the relative position of ethnic groups along a single continuum of ability, personality adjustment, or other blanket categories, but rather with the analysis of multidimensional group differences and the relationships of such differences to specific cultural factors. Such an approach is consistent with the results of factor analysis and with the growing emphasis upon special aptitudes rather than general intelligence in test construction. Straus (118) reports a striking discrepancy between the language and nonlanguage subtest scores obtained by 212 University of Ceylon students on the California Test of Mental Maturity. In terms of the American norms, the Ceylonese median fell approximately at the seventy-fifth percentile in the language scores and at the tenth percentile in the nonlanguage. These results are the reverse of those found with most bilingual groups. In explanation, Straus cites the high prestige traditionally enjoyed by verbal scholarship in Ceylon, as in other Oriental cultures, as well as the disdain felt by the upper-class Ceylonese for the type of manual, practical, and mechanical tasks which are related to good performance on nonlanguage tests. Reference is also made to the experimental form of a Ceylonese intelligence test, in which a similar but less pronounced discrepancy between verbal and nonverbal items was found.

Nonintellectual, or "personality," characteristics have likewise been investigated with reference to cultural differentials. Hsu (67) analyzed the responses of 144 Chinese students in America and 110 in China on a neurotic inventory. In terms of total score, the group living in China had the most "neurotic" mean, the means of the other subgroups decreasing with increasing length of American residence. An analysis of responses on each item, however, showed the futility of interpreting total score and revealed many interesting correspondences with characteristics of the Chinese culture. Culturally linked differences were likewise demonstrated in a study by

Smith & Vinacke (111) on the reactions of Caucasian, Japanese, and Chinese subjects in Hawaii to humorous stimuli.

In a survey of relocated Japanese-Americans in the Chicago area, Caudill (22) cites evidence indicating that the group made a remarkably successful vocational and general socioeconomic adjustment, in comparison with other minority groups in America. As a possible hypothesis, it is suggested that there is a significant compatibility between the value systems found in the Japanese culture and those prevailing in the American middle-class culture. To test this hypothesis, Caudill analyzed the Thematic Apperception Test records of 70 Japanese-Americans, 40 lower-middle-class Caucasian Americans, and 20 upper-lower-class Caucasian Americans. In these data, the author saw strong corroboration of his hypothesis. Because of the necessarily subjective nature of the interpretation of Thematic Apperception Test responses, however, such results can at best be considered only suggestive.

Further data on cultural differentials in personality development are provided by investigations of so-called "national character." An example is Farber's comparison of the traits named by American and British insurance clerks in completing the following statement: "The qualities I admire most in a person are . . ." (36). This is the first of a proposed series of articles analyzing the responses to 21 incomplete sentences. Mention should also be made of the extensive anthropological data on cultural differences in sexual behavior assembled by Ford & Beach in *Patterns of Sexual Behavior* (39).

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PERSONALITY¹

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Early in 1951, Leeper (62) in a provocative review of current personality theory took issue with the position that the most significant trend in modern psychology is "towards more objective concepts . . . based on operational definitions rather than on something of a more subjective sort, and concepts based on experimental evidence rather than on clinical material." While admitting that such a trend could be discerned, Leeper argued that it represented but a partial and superficial view of what was actually a movement in the opposite direction.

The main trend which I see as the most probable one into the future . . . is a trend away from . . . exclusive emphasis on tangible factors and towards an increasing appreciation and use of the relatively intangible. And it is a trend, in consequence, away from the emphasis on what is relatively infrequent towards emphasis on what is much more common and socially important.

In the opinion of the present reviewer, developments in the year following Leeper's statement vindicate him both as judge and prophet. Personality theory and research have indeed moved into areas which, like dreams, are at once both familiar and elusive. Originally concerned primarily with the uncommon and often bizarre characteristics of the individual person viewed in isolation, the personality psychologist of today is showing increasing interest in everyday people as they affect and are affected by familiar aspects of the everyday environment. At the same time, the attempt to deal—conceptually and empirically—with multiple, interacting relationships rather than simple static entities often leaves the theoretician with a system of ambiguous abstractions instead of a precise operational definition, and the observer-investigator with vague impressions instead of hard facts. As this writer has argued elsewhere (13), such abstractions and impressions are no less scientific for being, in their early stages, elusive and indeterminate. The tangible and

¹ The survey of the literature to which this chapter pertains was completed in May, 1952. Most of the material covered in the review was presented and discussed in a graduate seminar conducted by the author during the spring of 1952 and consisting of the following members: Bernice Borgman, John Butler, Walter Carel, Henry Landsberger, Edward McAlister, Pauline Moller, Vivian Olum, Stephen Richardson, David Smillie, Richard Suchman, Wayne Thompson, Adelard Tremblay, and Lee Wolin. The author is indebted to the members of the seminar for calling attention to many articles that might otherwise have been missed, for their challenging questions, and for many ideas which the reviewer, often unwittingly, has since incorporated as his own. Final responsibility for the reading, selection, and evaluation of materials covered in this review has of course been the author's.

operational are the objectives—not the conditions—of scientific progress. What is required of the scientist at each stage of his work is that he be as clear and rigorous as he can be at that particular time in relation to the particular problem under investigation. This means that in the development of new areas of inquiry, such as we see currently in the field of personality, we must constantly strive for precision but at the same time—to put it in the idiom of the day—be “tolerant of ambiguity” both in concepts and data.

The writer recognizes that this point of view is not shared by all of his colleagues, even those in the relatively “tender-minded” field of personality research. The orientation is made explicit so that the reader may better understand the basis for selection and evaluation of the materials treated herein. In addition, it should be noted that in the preparation of this chapter the selection of materials was guided by a conception of personality as a system of relatively enduring dispositions to experience, discriminate, or manipulate actual or perceived aspects of the individual's environment (including himself). Such a definition encompasses both phenomenal and, in Tolman's sense (107), nonphenomenal orientations. Thus it allows for unconscious as well as conscious dispositions. In Tolman's own words (p. 297), “it may include entities of which the actor is not then and there consciously aware.”

In line with the biases outlined above, the present review focuses primarily on disciplined attempts to conceptualize and study empirically the structure, function, and development of relatively enduring psychological dispositions. This means that nonsystematic essays or speculative case studies have been largely omitted. Also, since articles dealing primarily with pathology or deviant behavior and discussions of techniques and instruments are reviewed elsewhere in this volume, they have been treated here only in so far as they shed light on general theory and factual knowledge regarding personality. Finally, in view of the large volume and high quality of contributions to personality theory during the past year, it seems appropriate to divide the review into two parts, the first dealing exclusively with theory, and the second with research results.

THEORY

The increasing interest in personality theory reported in earlier volumes of this series reached a new high in 1951 to 1952. The outstanding publications include the provocative symposium on “theoretical models and personality theory” edited by Krech & Klein (60) and the report by Parsons *et al.* (80) of a signal effort of nine scholars representing different disciplines to achieve theoretical integration in the behavioral sciences. In addition, fully half the papers in the Pittsburgh symposium on *Current Trends in Psychological Theory* (22) deal explicitly with conceptions of personality, notably the chapters by Leeper (62), Krech (59), and Dennis (22). The most impressive and hopeful feature of these and other recent contributions is the fact that, though written by persons with widely divergent training and out-

look, the papers converge around a relatively small number of central problems.

Criteria for theoretical models for personality.—The first issue around which personality theorists converge in disagreement is the purpose to be served by the theoretical model. The argument—anticipated in the opening section of this chapter—is aptly summarized by Hebb (39).

There appears to be a left wing and a right wing in psychology . . . the Right favors parsimony of explanatory ideas, a simple or mechanical account of behavior, and definiteness even at the cost of being narrow. The Left is prepared to postulate more freely and can better tolerate vagueness and lack of system in its account of behavior.

Hebb himself, writing with a happy blend of scholarship and humor, vigorously champions the Left. Pointing to the positive value of "wrong theories" and "tautological constructs" as guides to observation and discovery, he baldly asserts:

It is perhaps a weakness of the human intellect that it must resort to such devices, but I think it is clear that thought is incorrigible in this respect. Thinking does not proceed according to formal logic even in natural science or mathematics . . . and attempting to act as if it did must be sterile.

The Right, however, is not without its militant defenders; in the same symposium Miller (75) forcefully restates the Hullian position:

A system of symbols . . . can properly be called a model or theory if, and only if, one can use it to make rigorous deductions about some of the consequences of different sets of conditions.

Feigl (27) and Eysenck (25) echo this view, and a number of other workers attempt to apply the general principle to the analysis of specific problems in personality theory. For example, Brown & Farber (14) strive to conceptualize emotions as intervening variables—an effort which to this reviewer seems premature and too far removed (in its elaborate logico-deductive symbols) from the realities of concrete behavior and experience. Appearing more successful are the attempts by Seeman & Buck (97, 98, 99) to formulate a behavioristic approach to the Freudian theory of daydreams and wish-fulfillment.

A second major point of convergence and conflict for the model-makers is specific to personality theory itself: What type of model shall be used? Bertalanffy (10) outlines the major alternatives and Tolman (107), using somewhat different terms, presents many of the same options. The principal issue at stake is the choice between a neurological versus a formal model. Emphasizing positions taken previously, Hebb (39) and Krech [joined by Klein (60)] continue to argue eloquently for a physiological paradigm, but for somewhat different reasons. Having made a case for the importance of new perspectives in theory building, Hebb contends that conscious physiologizing will enable the theorist to recognize and free himself from outdated tacit

neurological assumptions. Moreover, acquaintance with the new developments in neurological theory will stimulate new psychological conceptions. To cite his own metaphoric conclusion: "... an interest in neural anatomy and physiology may make more work for the midwife of psychological ideas than for the undertaker."

In view of the apparent similarity between the views of Hebb and Klein & Krech, a similarity which the latter themselves assume (p. 18), it is important to call attention to what appear to this reviewer to be basic differences in outlook. For example, in contrast to Hebb (who applies a gentle reprimand, p. 40), Klein & Krech offer as a major argument

that the inadequacy of neurology will be remedied in part by the attention the neurologist pays to psychological data and theory, and if he is slow to attend, then we psychologists must.

Similarly, their insistence that explanations must always be in terms of "new" (i.e., neurophysiological) variables independent of the observed event clashes sharply with Hebb's defense of the "tautological construct." Finally, they summarize their principal thesis as follows:

Psychological functions have no meaning apart from their being events of the physiology of the organism. Psychological constructs are only *translations* of these. Since this is so, any purely psychological theory will require retranslation eventually for complete understanding to occur.

Neurophysiology, it would seem, is the only reality in the psychologists' world, and since we have to get there in the end, we had better move there now. To their own argument that neurology is not yet ready to carry the weight of its sister science (or perhaps pseudoscience in their view), Klein & Krech summarily retort that it had better become so. All this seems, to the present reviewer, *sooner said than done*. To Klein who with Schlesinger (52) three years ago properly confronted psychologists with the question, "Where is the perceiver in perceptual theory?" one may justly retort "Where is the theorist in personality theory?" Surely in setting criteria for theoretical models we must consider not only the qualities of the ideal paradigm but also the needs, limitations, and special talents of the model-maker. It is in his willingness to do the latter that Hebb differs most sharply from his fellow champions in the cause of neurology.

The case for formal as against physiological models has its equally enthusiastic but somewhat less militant proponents in Bertalanffy (10), Tolman (107) and Parsons & Shils (80). The argument is entirely the pragmatic one; formal systems have "worked" in the sense of leading to crucial experiments (e.g., Mendel, Lewin). As to the question of choosing between the two types of models, Sheldon's (100) criticism of the General Statement signed by himself and his colleagues seems particularly appropriate:

I believe that one should make with considerable caution statements that such and such a set of categories *have* to be used. Considering the nature of theory in general,

one can never be sure that one's categories are the best possible . . . and the experience of science has shown that a set of data does not impose theoretical categories which have to be used. This is a matter of fundamental importance and is not hairsplitting.

At one point Klein & Krech state that "physiological conceptions are the most congenial and direct units for describing the organism." If they had added the words "for us," one could have no quarrel. Even Hebb's argument that psychological theories should not contradict what are currently believed to be neurological facts, does not seem crucial. In physics, the wave theory and corpuscular theory of light are contradicted both by some of the facts and by each other; their scientific usefulness, however, is beyond question. In our building of theoretical models for personality, let us by all means be aware of contradictions, but at the same time remain tolerant of them so long as they continue to lead to fruitful derivations and hypotheses.

The third point of focus for the model-makers involves convergence of opinion as well as attention. Such usually divergent theorists as Angyal (4), Bertalanffy (10), Brunswik (18), Krech (57, 58), Murray (77), Parsons (80), Sears (96), and Tolman (107) all agree in emphasizing the importance of viewing personality in terms of "open systems" permitting interaction between the organism and its environment. This requirement leads directly into the second major theoretical problem occupying a center-stage position during 1951 to 1952—the question of how to conceptualize the interactive system, how to supply the missing values in Lewin's (64) now-classical equation for personality theory: $B=f(P, E)$.

Conceptualizing personality in interaction.—In what is certainly the most impressive theoretical effort of the year (80) Parsons and his co-workers set as a major objective the development of categories for describing interrelationships among the three principal levels of analysis characterizing the behavioral social sciences; these three levels are the personality, the social system, and the culture. The merits and shortcomings of this brave endeavor are perhaps best illustrated by citing a definition of "orientation," the central unifying concept of the theory.

A specific combination of selections relative to . . . objects, made from among the possibilities of selection which were available in a specific situation, constitutes an orientation of action for a particular actor (p. 5).

The concept does permit the interrelating of personality, behavior, and situation and it incorporates both the phenomenal and objective properties of the field in which action takes place. At the same time, discussion is always on such a high level of abstraction that one is never quite sure just what is meant. Indeed, the General Statement of Chapter I, signed by all nine authors, functions almost as a projective test for the reader. One wonders whether ascent to the highest level of abstraction was necessary before all nine were able to reach the exalted plane of agreement. Nonetheless, the equally abstract chapters by Parsons & Shils in Part II are among the

most exciting in the book and worth the concerted intellectual effort they require. Not only do they compensate for the almost autistic conciseness of Chapter I, but the brilliant interrelating of concepts, and the ingenious derivation of manifold structures from the common base of the five "pattern variables" leave one with a feeling of profound respect and the belief that not only has the ground been broken, but the foundation actually laid for an interactive theory of personality-in-situation.

In Part III, Tolman (107), with his characteristic blend of gracious modesty and forthright individualism, gives due respect to his colleagues, and then proceeds to outline an interactive theory of his own. As MacLeod comments in a recent review (66), "Tolman is not one to retreat before a new idea. . . . He advances to meet it, with a gleam in his eye and another diagram to put on the board." To handle the problem of person-situation interdependence, Tolman develops the construct of the "behavior space," defined as the

particularized complex of perceptions (memories and inferences) as to objects and relations and the behaving self evoked by the given environmental stimulus situation and by a controlling and activated belief-value matrix.

While he does not explicitly recognize the parallelism, this formulation seems to correspond rather closely to Parson's concept of "orientation." Again, though Tolman comes down to earth more frequently, the high level of abstraction makes comprehension often a matter of conjecture. Indeed, this is the main criticism to be levied at the book as a whole. Nevertheless, this reviewer hazards the prediction that *Toward a General Theory of Action* will continue to influence personality theory for some time to come.

In addition to the extended expositions of Tolman and Parsons & Shils, a number of shorter papers on interactive personality theory require mention. Thus Parsons himself (79) reinterprets and extends Freud's concept of superego to make it consistent with action theory. Sears in his presidential address (96) offers a reformulation of a Hullian personality model in interdependent or, as he calls them, dyadic terms, and properly stresses the importance of the first, and somewhat neglected, term in the Lewinian equation: behavior.

There is no virtue in a descriptive statement that a person possesses such-and-such a trait or need unless that statement is part of a larger one that concludes with a specification of a kind of action to be performed. To describe a person as having "high emotionality" or "low sensitivity" or "diffuse anxiety" is systematically acceptable only if other variables are added that will, together with these internal personal properties, specify what kind of behavior can be expected for him under some specific circumstances.

Murray (77) presents a reorientation of his familiar theoretical views (76) in a chapter entitled "Toward a Classification of Interactions." He opens with the thesis that "the object must be given the same conceptual status as the subject," but his lasting interest and signal perceptiveness for the dy-

namics of the psyche soon force situational concerns into the background in favor of a careful re-examination of enduring needs in the structure of the person. Still, Murray's expressed dissatisfaction with his earlier dichotomy of need and press, and his adoption of the concept of vector as a means of linking action tendencies with values represent an important shift away from an attributive toward an interactive conception, a shift which, hopefully, will enable Murray to communicate more explicitly his almost uncanny ability to predict behavior from personality impressions.

At least one modern personality psychologist, however, remains unequivocally loyal to the psyche. In an attempt to develop a theory, consistent with the demands and findings of projective methods, Rosenzweig (90) proposes an "idiodynamic" approach in which the "stimulus is dethroned" in favor of a central interest in the "unique individual." In place of "stimulus-response relationships," Rosenzweig would have us construct our personality model solely in terms of "response-response relationships." As Frank (31) showed some years ago, and Allport before him (2), there is much to be said for the ideographic approach to personality theory and method. The view would indeed be consistent with the present rationale of projective techniques. But perhaps herein lies its greatest weakness. As forcefully demonstrated by the results of the Veterans Administration Research Project on the Selection of Clinical Psychologists (49), projective tests, when interpreted from the conventional perspective of purely internal dynamics, do not yield even minimally accurate predictions of performance. Nor are validity coefficients likely to rise so long as clinicians continue to rely on theories—explicit or tacit—which do not incorporate concrete conceptual bridges with the specifics of overt behavior and situational conditions.

In striking contrast to Rosenzweig, Foote (30) taking Mead (72) as his point of departure, proposes a "situational theory of motivation" which does away with dispositions altogether. "Criticism has negated every specific naming of 'the mainsprings of human action,'" Foote asserts, and we must "now boldly draw the indicated conclusion and deny wholesale the scientific validity of all such attempts. . . ." Instead, to provide motivational content for the "empty bottle of role and status," Foote offers the concept of identification. The term is used, however, in its literal rather than its psychological sense; that is, identification refers to the process of naming, of assigning an identity to the actor. To quote directly " . . . regularities in human behavior are organized responses to situations which have been classified more or less in common by the actors in them." It follows, says Foote, that not predispositions, but "names motivate behavior." It is one's sense of identity in the immediate situation that is "the key which unlocks the physiological resources of the organism, releases the energy . . . to perform the indicated act." But whence the lock, and what moving forces select the key and turn the tumblers? In short, where is motivation? One wonders whether Foote, in his dextrous manipulation of concepts, has not performed a sleight of hand and deceived even himself. Instead of filling the bottle, he has deftly changed the

label, leaving the flask as empty as before. Unfortunately for both Foote and Rosenzweig, one cannot solve the interactive equation by arbitrarily setting any one of its terms equal to zero.

That the equation can be solved, or at least written, without doing violence either to psychological or sociological realities is impressively evidenced by Sarbin (92), whose theoretical schema for analyzing the development of the self represents a skillful interweaving of the phenomenological tradition of James (46) and McDougall (70), the somatic-genetic theories of Freud (35), and the role concepts of Cooley (20) and Mead (72). To demonstrate that "the theory is not footless," Sarbin applies it to derive predictive hypotheses regarding different types of personality structure and associated behavior in a group of delinquent boys. An empirical study is reported under way and, when published, will permit a more definitive evaluation of Sarbin's plausible schema.

Finally, any discussion of interactive theory must take note of the republication in book form of selected contributions of W. I. Thomas to theory and social research (108). The editor's introduction is particularly useful as a systematic summary (lacking in Thomas' own writings) of the brilliant conceptions of personality-in-context which Thomas introduced and developed over a period of forty years.

Subsystems and personality structure.—While "interaction" is a rallying cry for psychologists of diverse persuasion when applied to interdependence between person and situation, use of the same word to describe relationships within the personality system itself has recently become a call to conceptual battle. Ever ready to respond to such a call, Krech (59) carries the offensive in attacking "separatism" in current psychological theory. His first target is what he regards as a false dichotomy between perception and motivation. Paraphrasing Hochberg & Gleitman (40), Krech decries the modern trend of setting up two sets of constructs and then positing "interaction" between the two (e.g. "perception as a function of need"). Such dualism, he contends, violates the law of parsimony and creates the false problem of interdependence of subsystems. What is needed, he asserts, and can now be attained, is not two or even one set of concepts, but "a single hypothetical construct which will do away with the cognitive motivational dichotomy." A few months later, joining forces with Klein (51) and firing much the same ammunition, Krech sets his sights on bigger game. The whole of personality theory is attacked for turning traitor to its original "protest against atomism" and becoming simply another subsystem along with "perception," "learning," "thinking," and the like. As a result, today—as in the early stages of psychology—one cannot "see the person for the subsystems." "Here then," say Klein & Krech, "is paradox and irony." But all is not lost; they also discern in modern psychology

a drive for systematization and unification of concepts . . . There is evident, for instance, a recent trend toward broadened conceptions of traits as "dispositional tendencies." . . . As a final development of this recent trend, some personologists

have come to realize that the problem of the "influence of personality traits" upon cognitive-motivational functions is essentially a pseudo-problem.

This "new" forward trend in personality theory noted by Krech & Klein is, in the judgment of the reviewer, perhaps even more of a "paradox and irony" than the "fall" which preceded it. The phrase "dispositional tendencies" calls to mind not the Allport of 1937 (3), whom Klein & Krech quote, but the McDougall of 1908 (70). When one recalls the latter's emphasis on the sentiment as a highly-organized "integrate" involving cognitive and conative-affective aspects, and his conception of the personality as implying—both in its development and structure—"a constant interplay between the self and society . . . always . . . one's self in relation to other selves," the formulations of today have about them something of the *déjà vu*. Thus the "theorists of action" (80) offer as their building stone for personality structure the concept of "need-disposition," defined as the "tendency to orient and act with respect to objects" and distinguished from "viscerogenic needs" by "its higher degree of organization and by its inclusion of motivational and evaluative elements." Likewise, Tolman (107) utilizes the concept of the "belief-value matrices" which he describes as "practically identical with that of need dispositions." Similarly, Murray (77) falls into the pattern with his redefinition of need "as a tendency to action" involving both a subject and an object. Finally, having dismissed Hochberg & Gleitman's (40) effort to unify psychology around perceptual constructs as "perceptual imperialism" and folly ("An attempt to force the 'hot' facts of life into the artificially-cooled Gestalt laws of perception will not work."), Krech (59) offers as his own nomination for the "single hypothetical construct" the concept of "dynamic system." Though his formal definition of the dynamic system is properly neurological (p. 130), his subsequent description of it has strikingly familiar overtones.

I have suggested, above, that Dynamic Systems are relatively enduring structures. That is, every one of you have a brain full of Dynamic Systems—Dynamic Systems relating to triangles and women, to science and magic, to paintings and fences, to chairs and professorships, to cabbages and kings—and you bring these Dynamic Systems with you whenever you enter any situation and your behavior reflects these Dynamic Systems as changed by the immediate situation. Thus whether you are discussing behavior with respect to women or science or paintings or chairs or cabbages, your behavior will show both variability and consistency in its emotional and motivational, and intellectual aspects.

The "latest models" of personality, virtually mass-produced by teams of highly trained specialists, resemble the original designs of a master craftsman working alone some forty years earlier. This is not to imply, however, that many improvements have not been made. Krech's attempt to establish firm linkage with neurological structure, Tolman's explanation of the relationships between the enduring belief-value-matrix and the "behaving self" in

the momentary field, and Parsons & Shils' (80) derivation of a universe of need-dispositions and orientations from combinations of five dichotomous "pattern variables"—all these developments, and others like them, permit application of theory to concrete research problems to a degree virtually nonexistent 40 years ago.

Before turning to such current research accomplishments, it is well to sound a note of caution regarding the much-praised current trend toward reductive integration—or as Lewin (64) called it, "homogenization"—in psychological theory. As Krech himself recognizes, any concept, however parsimonious and "beautiful," must "pay proper respect to the nature of our problems and data." Thus, a subsystem should not be postulated unless warranted by the demands of the problem and the data. Nor, this reviewer would add, should a subsystem be "unpostulated" when problem and data call for its application; and, from this point of view, the advocates of the "single hypothetical construct" may be clouding if not distorting the picture. For example, what of the empirical findings of Bruner & Goodman (16), Frenkel-Brunswik (1), and other "perception and personality" researchers? Without their shamefully dualistic conceptions, how are we to explain results or even conduct further experiments on "perceptual defense," "intolerance of ambiguity," and the like? Are they to be discontinued until such time as the "separatist" concepts on which they are based can be fitted into a single neurological model?

Before concluding the theoretical portion of this review, it seems appropriate to take cognizance not only of the recent points of convergence and heightened conflict in personality theory, but also of more placid currents set in motion by earlier theoretical storms. Prominent among these are developments in the several neopsychoanalytic schools, both orthodox and heretical. Since changes are more likely to occur in the latter, let us start there. Of the three most influential of the renegade disciples of Freud (Rank, Jung, and Adler), it is the last whose star appears to be rising most perceptibly on the current theoretical horizon. In at least two critical reviews (5, 21), he is singled out as the prophet and precursor of several important themes in current psychological thought. Also influential, though often not explicitly recognized, are the ideas of Rank (83), which appear strangely fated to generate theoretical offspring that no longer bear the family name. Thus the conceptual orientation of client-centered therapy (86) is essentially Rankian. Similarly, as this author has argued elsewhere (13), though disowned by Sullivan himself (103), Rankian ideas characterize much of the thinking of the late head of the Washington School of Psychiatry. Most recently, Angyal (4) presents "A Theoretical Model for Personality Studies" clearly patterned along Rankian lines, involving as it does "a two-directional orientation: *self-determination* on the one hand and *self-surrender* on the other." The orientation is persuasive [this reviewer can hardly say otherwise, having himself subscribed to a similar view (13)], but unfortunately (speaks the sobered voice of experience) is too general and ambiguous to be

translated easily into unequivocal and researchable hypotheses. Rankian conceptions, however, seem virtually operational by comparison when contrasted with the latest restatement of the theories of Jung. Jacobi (44) has prepared another edition of her periodic synopses of Jungian thought, making available to the English reader the new developments in this Swiss school. Except for those who are familiar with and accept Jungian theory, however, the book is not likely either to communicate or convert. Finally, to turn to the more orthodox neo-Freudian tradition, Rapaport (84) as his contribution to the Klein & Krech symposium, presents "The Conceptual Model of Psychoanalysis." Again a prior identification with the concepts is probably necessary for the reader to gain maximum profit from this somewhat extended schematic analysis.

In contrast with and in deprecation of Freudian approaches, Eysenck (25) allies himself with the self-styled "tough-minded" tradition in psychology by presenting what is in effect a "factor-analytic" model for personality. A similar approach is offered by Halstead (37), who analyzes basic personality structure in terms of three factors, presumably of constitutional origin, which comprise what he calls "biological intelligence." Although Halstead promises to develop "the possible biosocial extensions of the model," the reviewer finds it difficult to see how this can be done so long as a noninteractive theoretical framework tacitly dictates both what is put into and inferred from a factor analytic design. The same criticism applies in part to Cattell & Winder's (19) factorial approach to the conceptual analysis of rigidity, although the distinction between rigidity in "new" versus in "repeated" situations seems a step in the right direction.

Occupying a middle range between "tough-" and "tender-minded" theory are advocates of the frustration-aggression approach to personality dynamics. Among these, Klee (50) is noteworthy for his reinterpretation of "frustrated" behavior in rats and in humans, as the selective, adaptive, best possible response (viewed from the subject's perspective) under the given difficult conditions. Again, the line of thought is reminiscent of Rank (83)—in this instance, of his reinterpretation of resistance and neurotic defense as reflecting the creative potential of the organism. A more conventional view of frustration appears in theoretical contributions by Thetford (105) and Arsenian (6) to a symposium inaugurating a new quarterly (110) dedicated to personality theory and research.

Finally, to end on a classical note, there are a number of orientations that have become so much a part of personality theory, past and present, that they are taken for granted and identified by source only in periodic tributes and re-evaluations. Thus, to cite but two examples, in 1951 to 1952 we find Dennis (21) again re-emphasizing the importance of Piaget's thinking to American psychologists, and, as if in response, a translation of Piaget's *La Formation du Symbole* has appeared for the first time in English under the title *Play, Dreams, and Imitation in Childhood* (81). Lastly, in the long-established British tradition of scholarly criticism, Smith (102) re-examines

the contributions of McDougall, Lewin, Allport, Watson, Hull, and Tolman to the theory of human behavior. An opportunity to view the six systems side by side in well-organized form but yet faithful to the original, merits special mention, particularly since the book is not available, at least as yet, in an American edition.

RESEARCH

Having outlined the major trends in contemporary personality theory, it is reasonable to ask how they affect and are affected by current empirical investigation. Setting forth on this task, the present reviewer is once again confronted by the challenge of his own biases. He has looked for research studies, even of the most primitive sort, that could be regarded as at least inspired by, if not actually based on, a neurological model, but has found—or perhaps better, seen—none. True, it may be too early to expect this “newest look” in personality theory to have affected actual research operations. The more likely alternative, however, is that the reviewer’s perceptual defenses have been faithfully at work, for he has had no difficulty in finding evidence to support the research potential of those theoretical trends which he himself espouses or approves. Thus, investigations based—explicitly or implicitly—on formal models abound in every quarter, and the majority of current empirical studies seem to focus around the general problem of reciprocal interplay between personality, behavior, and situation.

Person behaving in a situation.—Taking the first step from theory toward application, Freedman and his colleagues (33) offer “a comprehensive schema for the organization of personality data.” The present paper deals with two of four types of personality variables: the interpersonal mechanism and the interpersonal trait. (The remaining two intrapersonal variables are to be presented in subsequent publications.) The interpersonal mechanism is virtually equivalent—both theoretically and methodologically—to Bales’ unit of the “interact” (7); that is, each person is scored for the types of concrete behavioral acts he exhibits toward others, and the sum of scores for all persons present provides an index for the situation as a whole. In this way, say the authors, a quantitative “field theoretical” picture is achieved. The interpersonal trait represents “the potentialities for interpersonal action perceived by individuals” and is measured by subjects’ ratings of self and others on adjectives corresponding to each of the verbs used to classify the interpersonal mechanisms. Applications of the measures to different types of material (therapy transcriptions, plays, projective protocols, etc.) are shown and satisfactory inter-rater reliabilities are reported. One wonders about the possible cumbersomeness of a method involving 16 categories with three intensity ratings in each, but here, as with the schema as a whole, an appraisal of the value of the technique waits upon its use in the investigation of concrete hypotheses.

At the opposite end of the research funnel, is the final report of the Veterans Administration Research Project on the Selection of Clinical Psychol-

ogists (49). The document adds little to specific substantive conclusions, which have already been reviewed in the present series (26). With the total product for the first time before us, however, it seems appropriate and even obligatory to consider the contribution of this expensive type of program research to the advance of basic theory and knowledge. The results of such consideration are—to use the authors' own term—indeed “sobering.” But, as one who participated in the effort, the present reviewer is less appalled by the “devastating” implications for clinical theory or practice stressed by the authors and others (26), than by the implications for the research itself. A tremendous investment of money, time, and effort on the part of a score of America's best clinicians and researchers has yielded, so far as the reviewer can see, virtually no substantive findings regarding personality structure, function, or development. What little we can say regarding the personality correlates and antecedents of professional clinical performance can be said, it seems, on the basis of a few “tried and true” objective tests, and even these do no better than the information routinely supplied on an application blank for graduate school. For clinicians to accept all the responsibility for these negative findings seems logically unjustifiable—even if required by their allegedly intropunitive personality structure. Clinical skill was just one element in a research design characterized, among other things, by virtually complete absence of commonly-agreed-upon hypotheses to be tested. Everyone was left to provide his own “personal equation,” first for linking observed behavior with personality traits, and then for reversing direction and predicting future professional performance in an unspecified situation. (As one who participated in the planning of the research design, the writer must accept his full share of responsibility for this theoretical vacuum.) The absence of explicit systematic theory is perhaps excusable in applied research, but not when the results are used to call theory into question. As indicated elsewhere in this review, the tacit assumptions underlying much of current clinical practice are far from adequate, but prediction of future professional success is hardly a fair test of a personality theory—however poor it may be.

Indication that the theory underlying projective tests is not completely inadequate even in its present form is given in a study by Swanson (104), who reports statistically significant predictions of the social behavior of group members from blind analysis of Blacky Test protocols. Of particular interest is the fact that situational conditions were taken into account. Utilizing two groups, A and B, the test-interpreter made predictions for Group B members first “as if their total participation were determined by the ‘atmospheric forces’ in Group A” and then as if affected “by the less supportive atmosphere” of Group B, of which the subjects were actually members. The resultant rho correlations ($N=17$) were $-.11$ and $+.56$ respectively. Unfortunately, the specific “coordinating hypotheses” utilized by the analyst in predicting behavior from inferred personality structure and situational climate are again not spelled out. This difficulty is remedied on the side of personality in an ingenious and closely related study by Miller &

Stine (74), who obtained a striking positive correlation (for elementary school children) between sociometric status and indices of pregenitality. The latter were secured from objectively-scored content analyses of a specially-designed incomplete stories test. Particularly noteworthy are two exceptions to the general trend: (a) Analylty scores showed a negative relationship with sociometric status (suggesting, say the authors, that in American culture anal character traits may be an asset during the latency period); (b) Isolates obtained many of the highest as well as the lowest pregenitality scores (these, it is proposed, may be potential leaders too mature for the group).

The researches on the achievement motive directed by McClelland (68, 69) continue to yield useful data regarding the interrelationship of personality needs and behavior; in an experiment by Lowell (65), college students with high need-achievement (as measured by an objectively-scored projective test) showed higher output in simple tasks (unscrambling words, adding two-digit numbers) than an otherwise comparable low need-achievement group. The use of a dynamic molar unit of analysis to demonstrate relationships between needs and behavior contrasts sharply with negative findings customarily obtained when narrowly-defined static categories are utilized. The study by Brozek *et al.* (15) of the personality correlates of experimental semistarvation is a case in point; the projective tests were analyzed principally in terms of responses related directly to food or eating, an unfortunate limitation from the point of view of personality theory.

Also working with college students, Mussen & Wyszynski (78) report significant relationships between measures of political participation (based on information supplied by the subjects) and analyses of responses to projective questions. On the basis of their data, the investigators conclude that:

Political apathy and activity are manifestations of more deep-lying, and pervasive passive and active orientations. Thus, one of the outstanding characteristics of the politically active individual is his attempt to understand himself, i.e., his awareness, examination, and acceptance of his own emotions, conflicts, and feelings, including feelings of inadequacy and inferiority. . . . The politically apathetic individual, on the other hand, seems to be generally passive, dissatisfied, and generally threatened.

The authors do not restrict their generalizations to the college population on which they were based. Of interest in this connection is the fact that the politically active and apathetic groups did not differ significantly in scores on items from the Anti-Semitism, Ethnocentrism, Politico-Economic Conservatism, and Authoritarianism scales of the California study (1), even though presumably some of these scales differentiate between the two polar personality syndromes which the authors describe. This raises the general question of the role of situational factors in influencing responses presumed to reflect basic personality structure.

We may take note briefly of two descriptive accounts of personality viewed in its larger social context: the report by Jaques (45) on *The Changing Culture of a Factory* and the personality studies by Joseph & Murray (47) of the *Chamorros and Carolinians of Saipan*. Both of these books share the

typical virtues and shortcomings of the case study. Down-to-earth, insightful, and persuasive, they lack the systematic methodology which would permit nonequivocal inference. This is particularly true of the Rorschach data presented by Joseph & Murray. Let us hope that the day is not far off when some one will do for the ink blot what McClelland has already begun to do for the picture-story test—provide a reliable and experimentally validated scoring procedure.

Overt and covert needs versus environmental pressures.—During 1951 to 1952, two originally independent groups of investigators have converged to work a rich lode for data bearing on the interactive problem. The first group, taking as its point of departure the frustration-aggression hypothesis and struck by the seeming exceptions to the rule, has sought to investigate the particular conditions giving rise to and permitting the overt expression of aggression. The second group, stimulated primarily by problems in the interpretation of projective data, has attempted to explore systematically the relationship between such data and antecedent and consequent behavioral events. The convergence of trends is neatly illustrated in an experiment cited by Sears in his presidential address (96).

A simple example of the measurement problem created by these [interactive] considerations arose in connection with some data on aggressive behavior collected in our laboratory. Forty preschool children were the subjects. Two main measures of aggressiveness were secured. One was overt and socially directed aggression. This measure was obtained both by teachers' rating scales and by direct observation. The other was projective or fantasy aggression displayed in doll play. By a fixed trait or need assumption, one would expect these two measures to correspond somewhat. They did—somewhat! The correlation was $+.13!$

The seeming paradox is resolved, Sears points out, when measures of aggression are related to indices of severity of the mothers' punishment of the children's aggressive acts at home. The general hypothesis, confirmed by two workers in Sears' laboratory, Hollenberg & Sperry (41), is stated as follows:

... punishment of aggression (a) decreases the frequency and intensity of aggression in the situation in which punishment occurs, and (b) increases the frequency and intensity in situations distinctly dissimilar to that in which the punishment occurred.

Results consistent with this hypothesis are reported by investigators working with different problems and utilizing a variety of projective techniques; e.g., Meltzoff (73), Graham *et al.* (36), Zuckerman (111), Holzberg & Posner (42), Fisher & Hinds (29), and Korner (56). The last study is particularly interesting for, though apparently done independently, it paralleled closely both in methods and results the experiments of Sears and his co-workers (95). At the same time, lacking an interactive frame, Korner comes to a completely negative conclusion ("the relationship between overt and covert mental activity is unpredictable") and seeks an explanation in terms of "a triple assessment of personality layers" within the psyche. A similar regression in depth is advocated by Fisher & Hinds (29), who base their conclu-

sions on a correlational analysis of eight tests designed to tap levels of hostility control in various personality structures. Such conceptions are not entirely without justification (provided they are offered with due respect to the law of parsimony), for the question remains of what happens to hostile impulses that cannot find overt expression. The research results would seem to indicate that they are somehow retained within the personality structure and have particular effect on the perceptual process.

Personality and perceptual defense.—The relationship between needs and behavior is being further clarified by the ever-growing number of experiments on "perceptual defense." While they focus primarily on projective material, these experiments shift the emphasis from environmental conditions to personality structure itself. Representative of several such studies is the series of papers by Eriksen and his colleagues (23, 24, 61). Eriksen first clears the ground by reversing the common assumption and proposing that "projective tests are likely to reveal only non-repressed needs, not those that are repressed"; in confirmation, he finds that subjects with low recognition thresholds for aggressive stimuli (pictures) give more "aggressive" Thematic Apperception Test stories than subjects with high recognition thresholds. Repressed needs, he suggests, are more likely to be revealed by blocking, distortion, or other signs of disturbance. Eriksen secures strong support for this second hypothesis in a subsequent experiment utilizing a word association technique (24). Here significant positive correlations are obtained between disturbance scores and "degree of perceptual threshold elevation for corresponding need scenes." Further corroborative evidence for these conclusions is reported independently by Rosenstock (89), Williams (109), and Lazarus *et al.* (61). The last group of investigators, working with a sample of outpatients, take the additional and usually rewarding step of examining exceptions to the general trend, with resulting implications for research on differential character structure.

Two basic reactions to threatening stimuli were found: (a) high perceptual accuracy and ready verbalization [on a sentence completion test]; and (b) low perceptual accuracy and minimal verbalization with blocking. Individuals were found to be consistent in their use of these basic reactions.

Data in line with this analysis are also reported by Belmont & Birch (9), who, in a study of experimentally-induced repression, were able to differentiate significantly two groups of subjects—those who learned shock syllables more quickly than neutral material and those who learned them more slowly. Analogous differences were also observed on recall and relearning scores.

The finding by Belmont & Birch (9) that one group of subjects learned shock syllables more quickly than neutral material calls into question a common tendency in personality theory to identify motivation with "wish fulfillment" (e.g., pleasant stimuli are recognized and learned more quickly than unpleasant). This tendency is singled out for direct attack in an experiment by Postman & Leytham (82), who demonstrate that names of traits

which are important to the subject (as measured by discrepancy between his self-rating and estimate of how others would rate him) are equally well recognized (when presented tachistoscopically) irrespective of whether they are regarded as desirable or undesirable by the subject. [The implications of the data for the Bruner-Postman "hypothesis" theory of perception (17) are not considered here.] It is regrettable that Postman & Leytham do not follow the example of Lazarus and his colleagues in exploring the exceptions to the rule. As emphasized recently by Klein *et al.* (54), it would be in the mutual interest of both the fields of personality and perception to consider

reconceptualizing the problem of values and needs in perception, and rooting it in the study of individual variation, perceptual reorganization, and ego structure rather than solely in terms of motivational theory.

Character structure and development.—What was once a no-man's-land between personality and perception is now overpopulated by homesteaders who do not always welcome inquiries about their former address. Nevertheless, Frenkel-Brunswik (34) has ventured to distinguish two groups among the new arrivals in terms of their point of origin. There are the "personality-centered" psychologists who start with the total human being, and the "perception-centered" psychologists who, she hopes but is not sure, will end there. Since she is one of the leading exponents of the former approach, it is not surprising that when we turn our attention to researches explicitly concerned with types of personality organization rather than isolated processes, we find a preponderant number of studies which acknowledge as their source the now-classical report on the *Authoritarian Personality* (1) prepared by Frenkel-Brunswik and her colleagues. A representative example is an experiment by Block & Block (11), who obtain significant positive relationships between the tendency to establish norms in an autokinetic experiment and scores on the California Ethnocentrism Scale. Moreover, in a subsequent study (12), they show that the subjects who do not establish norms are also those who are most ready to resist seemingly-arbitrary suggestions from the experimenter. The second study is particularly welcome in supplying evidence—far too sparse to date—of the behavioral correlates of authoritarian personality structure. Also, as the authors point out, it sheds light on the meaning of the experimenter to the subject—a variable which is far too often overlooked in the usual laboratory experiment. Finally, one wonders about the possible relationship between "intolerance of ambiguity" as measured by Block & Block and "perceptual attitudes toward instability" as determined by Klein & Schlesinger (53). Selecting two groups of subjects on the basis of "form-boundedness" versus "form-lability" as evidenced in Rorschach responses, the latter investigators find significant differences in the ability to see apparent movement (phi-phenomenon).

In another comparison between high and low scorers on the Ethnocentrism Scale, Fisher (28) examines the relationship between ethnocentrism and "memory changes" observed in successive reproductions of asymmetrical

test figures. In line with his hypothesis, the "highs" showed greater distortions in the direction of simplification and symmetry, while the "lows" were better able to "alter the course of Prägnanz" and to preserve the idiosyncrasies of the original design. This investigation is nicely complemented from the situational side by Korchin *et al.* (55) in a study patterned after Barker, Dembo, & Lewin (8). The subjects, 18 resident psychiatrists and psychiatric social workers in a mental hygiene clinic, were asked to reproduce a series of sample designs before and after a frustrating task. When compared with a control group, the experimental subjects showed reduced accuracy and greater "primitivation." (The latter term is virtually equivalent with Fisher's "tendency toward symmetry.") A comparison of these two polar approaches to the problem of perceptual regression suggests that an experiment in which both sets of variables were dealt with simultaneously would be of considerable significance for an interactive theory of personality structure, function, and development.

Still another disciple of the California school, Rokeach (87), proposes concepts and methods for differentiating three stages of "cognitive organization along a single continuum ranging from *comprehensive* to *isolated* to *narrow*." Although no explicit reference is made to the possible parallelism, the three concepts as defined are strikingly similar to the W, D, and Dd of the Rorschach test. In a second study (88), Rokeach shows significant correlations between scores on the California Ethnocentrism Scale and the three types of cognitive organization with high scorers showing a comprehensive approach; middle scorers, isolated; and low scorers, narrow. Relationships between type of cognitive organization and actual social behavior are not discussed.

The second major source for hypotheses regarding character structure and development is of course Freudian theory itself. Taking this as his point of departure, Sarnoff (93) investigates personality organization in Jews displaying high scores on a test of anti-Semitic attitudes developed by the experimenter. The general hypothesis, derived from the theory of the Oedipal conflict, is that the anti-Semitic Jew is an example of "identification with the aggressor"—a defensive taking-on of the orientation of the punitive, rejecting parent figure. Applying this paradigm to the social situation of the Jew in the contemporary American scene, Sarnoff selects and confirms three hypotheses on the basis of significant differences between objectively-scored projective protocols of high and low anti-Semitic groups of Jewish students. Specifically, the former gave evidence of introjecting the attitude of the aggressor in manifesting more negative attitudes toward their parents and themselves and being more "passive in the face of interpersonal hostility." Sarnoff contrasts these findings with those for the non-Jewish bigot of the California studies (1) who is extrapunitive rather than intropunitive and who overtly glorifies his parents and himself. It is regrettable, as he himself recognizes, that Sarnoff was not able to include non-Jewish anti-Semites in his study. One also would like to see the Jewish bigot's response pattern on the

California scales, and, above all, the ever-missing data on the actual behavior of high and low subjects in real-life social situations.

Also rooted in the Freudian tradition is Harlow's highly imaginative study (38) of the personality structure of weight-lifters as an example of compensatory reaction to masculine inadequacy. A group of 20 weight-lifters is compared with a control of non-weight-training athletes on two projective techniques scored independently for 18 specific hypotheses derived from psychoanalytic theory; 13 of the 18 show significant differences. For example, the weight-lifter reveals stronger feelings of masculine inadequacy, narcissism, hostility toward the mother, homosexual impulses, etc. The findings are impressive, but one is left with questions regarding their specificity to weight-lifters as such. The success of Harlow's modest but disciplined effort contrasts sharply with the inconclusive effect of an extensive investigation by Roe (85) of another, and, unhappily, more important special-interest group—that of 22 eminent experimental physicists. Utilizing a conventional comprehensive clinical approach (e.g., life history interview, Rorschach, Thematic Apperception Test, etc.), with no explicit hypotheses or experimental design, Roe leaves us with little beyond admiration for recognizing an important problem and a renewed conviction in the practical utility of theory and of an ordered delimitation of hypotheses to be explored.

Moving now from a concern with character structure toward problems of personality development, we again find Freudian theory playing the dominant role in the posing of research questions. The studies of Miller (74) and Swanson (104) examining the social correlates of pregenital and pre-Oedipal fixations have already been discussed. There remain for consideration a series of researches dealing with the effects of infantile experience on subsequent behavior. With one exception, these are all animal experiments done at the Roscoe B. Jackson Memorial Laboratory, the principal workers being Fredericson (32), Kahn (48), Scott (94), and Ross (91). The last paper presents a summary of the program of research at the Laboratory, a program which seems highly valuable and promising, for not only does it extend and place on a sounder experimental foundation the findings of pioneer studies like those of Levy (63) and Hunt *et al.* (43), but it also introduces explicit consideration of an often-uncontrolled and even unrecognized variable—the interpersonal relationship between the animal and the experimenter. Viewed in this sociobiological perspective, animal psychology may be expected to become increasingly meaningful for personality theory and research.

In the one study utilizing human subjects, Thurston & Mussen (106) report negative findings. There is some question, however, whether the results actually apply to the relationship the investigators believed they were testing. Their putative hypothesis called for positive correlations between various needs and presses scored from Thematic Apperception Test protocols and indices of oral deprivation in infancy. It is the latter which call the research into question inasmuch as they are based exclusively on responses to mailed questionnaires by the mothers of the student subjects. Even granting the

desire to give truthful replies, one can certainly question the validity of a mother's memory for feeding schedules maintained almost 20 years earlier. Yet this question is not even raised by the investigators. Nor is reference made to the closely parallel study yielding positive results by Maslow & Szilagyi-Kessler (71). Moreover, both of these experiments on adults are cast in a different light if one follows the lead of current personality theory and views the retrospective reports by the mother not only as a function of factual recall but also as a reflection of the mother's present perception of her role vis-à-vis the child.

So much for specific reports of current theory and research. It has become customary, at the conclusion of a review chapter on Personality in this series, to comment on new general texts in the field published during the year. Two such books have come to the attention of the reviewer. *Personality Development* by Slotkin (101), though aspiring to utilize "the hypotheses and evidence from various relevant sciences" is primarily anthropological in orientation and gives psychology short shrift—as evidenced by the omission from the index of such names as Stern, Piaget, and Lewin, let alone Werner, Frenkel-Brunswik, and E. H. Erikson. Far better balance is evidenced, at least from the perspective of the reviewer, in McClelland's *Personality* (67) where clinical and experimental materials are skillfully interwoven around the central theme of a case study of Karl, a 24-year-old college student. The essentially cross-sectional, or at best retrospective, orientation of the book, however, does not give the reader adequate knowledge or appreciation of forces and stages in the process of growth. The textbook is yet to be written which successfully combines the developmental and social-anthropological approach of Slotkin with the clinical-experimental perspective of McClelland. Perhaps our knowledge and wisdom are not yet equal to the task.

The last statement prompts the final question of this review. Having concluded an appraisal of the year's accomplishments in personality theory and research, one naturally asks: how far have we come, and where are we going? Somewhat to his own surprise, this reviewer finds himself heartened and impressed. We are indeed moving, as Leeper has suggested, toward creating tangibility for what is at once elusive, familiar, and socially important. By a paradoxical resort to abstraction and complexity, we are finally coming down to earth. In the process, the psychology of personality has moved far from what it once was—a jumble of disconnected, often irrelevant facts and fragmentary, footless speculations. To borrow Rokeach's terminology, we are no longer "narrow-minded" either in the theory we spin or the data we collect on the web; there are still some "isolated" areas, but, by and large, we are moving toward a "comprehensive" picture—a picture characterized not by the sterile sameness of a single system of constructs but by that paradoxical pattern of congruities and contradictions which is the challenge and hallmark of a developing science.

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES¹

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SYSTEMATIC PRESENTATIONS OF THE FIELD OF SOCIAL PSYCHOLOGY

Of the 16 textbooks in social psychology published since 1947 (not including collections of readings), no less than six have appeared during the 12 months covered by this review. Textbooks necessarily lag somewhat behind contemporary research findings, but they are often in the forefront in respect to systematizing the fields which they represent. An examination of the 20 index items under which the most frequent entries appear (excluding contentless items like "research" and "hypotheses") has been made for five of the six texts.²

A total of 68 items was counted, each of which was among the 20 appearing most frequently in one or more of these texts. No single item appeared in all five of them; the frequencies were as follows: 3 items appear in 4 texts (attitude, culture, perception); 6 items appear in 3 texts (group, leader, learning, motive, personality, role); 11 items appear in 2 texts (crime, drive, emotion, ethnic, instinct, intelligence, religion, self, social class, socialization, status); 48 items appear in only one text. (It may or may not be significant that this distribution rather closely follows "Zipf's law": i.e., the product of the item frequency and the square of the number of texts yields an approximate constant.) At this level of analysis each author shows a good deal of individuality; in every case about half (from 9 to 11) of the 20 most frequently appearing items are idiosyncratic. Many of these items, however, can readily be categorized together. It appears possible, in fact, to classify all of the 68 items under 10 headings, as follows: (a) Psychological states, properties, processes; (b) social structure, social groupings, institutions; (c) individual personality, self; (d) socialization and developmental processes; (e) social attitudes; (f) uniformities within groups, group behavior, social norms; (g) role differentiation, leadership; (h) interaction and communica-

¹ The survey of the literature to which this review pertains was completed in May, 1952. This is necessarily a selective review, since it was not possible to make a genuinely exhaustive coverage of all the relevant literature during the period under review. Guiding principles in selection have inevitably included the reviewer's areas of familiarity and interest; in addition, there has been a deliberate attempt to organize materials around a few major problems. Hence a certain number of contributions which other reviewers might have included—some of them highly significant, perhaps—are omitted.

The assistance of Leonard M. Lansky in combing the relevant literature is gratefully acknowledged.

² The volume by Asch (3) appeared too late to be included.

tion within groups; (i) social problems; (j) research methods. All but (h) and (j) of these appear, in one form or another, among the most frequent index entries of two or more of the five texts considered.

Treatments of (a) and (b) draw heavily upon individual psychology, sociology, and anthropology, while (i) and (j) deal with applications and methods. It is thus the remaining six topics which carry the specifically social psychological load, and of these (c) and (d) overlap considerably in content (though not necessarily in manner of treatment) with textbooks in personality, child development, or sociology. The distinctively social psychological content is to be found in (e), (f), (g), and (h).

According to this very crude system of content analysis, the distributions of most frequent index entries in the five volumes are as shown in Table I. It should be remembered that individual psychological and sociological concepts are usually introduced for social psychological purposes, and the treatment of such concepts is not necessarily the same as it would be in introductory texts of the relevant disciplines. For example, the chapter titles "The inadequacy of biological motivation" and "Social determination of learning, perception and memory" (Faris) would hardly be found in introductory texts in psychology.

TABLE I
INDEX FREQUENCIES IN FIVE TEXTBOOKS

Category	Dewey & Humber (30)	Doob (32)	Faris (36)	Hartley & Hartley (53)	Queener (93)	Total
<i>a</i>	5	4	13	3	3	28
<i>b</i>	4	5	3	4	4	20
<i>c</i>	3	1	2		2	8
<i>d</i>	2	1		1		4
<i>e</i>	2		1	2	1	6
<i>f</i>		3		3	2	8
<i>g</i>		1	1	2	3	7
<i>h</i>		1		3	1	5
<i>i</i>	4	3		2	3	12
<i>j</i>		1			1	2
Total	20	20	20	20	20	100

This point is further illustrated by the fact that the two texts authored in whole or in part by sociologists (Dewey & Humber; Faris) are the two whose references to individual psychological concepts are most frequent. Conversely, the other three, all authored by psychologists, include more sociological than psychological concepts among their most frequent index entries.³ This somewhat surprising finding may be interpreted by assuming

³ Lest the reader suspect that some mistake has been made, the 20 most frequent index items from two books are as follows, verbatim: *List A*: crime, crowds, culture, ethnic groups, fads, family, groups, interaction (primarily of stimulus and response),

that each author tends (a) to assume that his audience needs elementary instruction in the field that is not his own specialty, and (b) to interpret the data and/or concepts from the other field in terms of the theories of his own field.

Two of the volumes are straightforwardly presented in psychological frameworks. Thus social psychology is "the study of individual behavior in response to stimuli from other individuals" (Queener); and "social psychology serves the interdisciplinary function of connecting a knowledge of people's responses with a knowledge of the stimuli that contribute to the production of those responses" (Doob). Two others (Dewey & Humber; Faris), intermediate from the point of view of the present classification, adopt a social-interaction point of view, but without particular emphasis upon specific processes of interaction.

The two remaining textbooks of the year are very explicit in pointing to social interaction as the distinctive subject matter of social psychology. For them, interaction is to social psychology as individual behavior is to psychology. Thus social psychology is "focused on those aspects of social behavior found when men interact with one another . . . searches for the universals to be found in human interaction" (Hartley & Hartley). Social psychology, according to Asch,

has a subject matter of its own . . . it needs to describe its own phenomena and to discover its own principles. . . . men in interaction with each other form invariant properties which cannot be reduced to the traditional categories of habit or instinct.

Finally, the 1952 revision of *Readings in Social Psychology* (109a) most conspicuously departs from the 1947 edition, which was widely used as a textbook, in presenting an organizational framework in terms of interaction patterns.

It is important to remember that, with or without the use of the word, social psychology has always been defined in interaction-like terms. As early as 1931 Folsom (43) defined social psychology as "the study of the action of human personalities upon one another," and devoted three of 14 chapters to "interaction." Katz & Schanck, in 1938 (73), devoted two of 20 chapters to the same topic. Young, in 1944 (117), was perhaps the first to define the field of social psychology explicitly in terms of interaction. What characterizes the present trend, if trend it be, is not so much the use of the term as the recognition that the term stands for something which can be studied at its own level. It is increasingly being insisted, in fact, that it must be so studied, since no mere extrapolation of general psychological principles can adequately account for the observed facts of social influence.

language, leaders, motives, perception, personality, predispositions, religion, social change, social class, socialization, surveys, war; *List B*: ability, attitude, canalization, conditioning, consciousness, drives, emotions, instincts, intelligence, learning, memory, motive, neurosis, perception, primary group, repression, roles, self, social control, status. The former list is from Doob, a psychologist, and the latter from Faris, a sociologist.

The seven general books of the year under review, in spite of their differences, have an immense advantage over their prewar predecessors—a considerable body of empirical data dealing with conditions and consequences of human interaction. Thus it is becoming possible to test, to use, and to develop further a body of concepts and interrelated propositions upon which sound theory rests.

ATTITUDES AS RELATED TO INDIVIDUAL PSYCHOLOGICAL VARIABLES OR PROCESSES

This review will concentrate upon investigations in which the effects of some sort of influence have been studied, either "in the field" or in the laboratory, so that changes can be related to an independent variable, and upon correlational studies. It will for the most part ignore the considerable number of studies of "audit" type—i.e., those whose objective seems to be to present distributions of attitude within a certain population, with or without demographic breakdowns.

Attitude change.—The most comprehensive among the current research programs in attitude change is probably the one under the general direction of Carl Hovland at Yale University. A study by Janis, Lumsdaine & Gladstone (67) in this series provides substantial support for the hypothesis "that any communication which has successfully modified a person's belief will reduce the opinion-impact of any subsequent event . . . that tends to produce antithetical beliefs." This "interference" hypothesis was tested by presenting to classrooms of high school students an "optimistic" statement, by "an expert on the subject" who spoke "on the radio," to the effect that "Russia will *not* be able to produce A-bombs in large quantities *at least for the next five years.*" Questionnaires, including the item, "About how long from now do you think it will be before the Russians are really producing *large numbers* of atomic bombs?" were responded to two weeks before and two weeks after the experimental influence. Three months later President Truman announced that Russia had exploded an A-bomb, and four days later the questionnaire was administered for the third time. A control group heard a "radio" program on a completely different subject. Various comparisons of the final responses seem to justify the authors' conclusion:

. . . the radio program which had been presented three months earlier had an observable carry-over effect, producing a significant degree of *resistance* to the opinion-impact of the event. . . . the "optimistic" radio program had the effect of *reducing* the amount of "pessimism" evoked by the subsequent event.

In another study, interpreted by the authors in terms of learning and forgetting theories, Hovland & Weiss (60) studied the effects of source credibility upon acquisition and retention of communicated material by presenting to a class of college students a printed article on each of four different topics, articles being attributed to such various sources as *Fortune* and *Pravda*. The degree of change in opinions immediately after reading the

materials varied as a function of subjects' ratings of "trustworthiness," but retention of factual information was not related to credibility, either immediately or four weeks later. There was, however, a decrease, four weeks later, in the extent to which subjects agreed with the position advocated by trustworthy sources, and an increase in agreement with untrustworthy sources ("the sleeper effect").

If resistance to acceptance diminishes with time while the content which itself provides the basis for the opinion is forgotten more slowly, there will be an increase after the communication in the extent of agreement with an untrustworthy source.

Another study in this program, by Feshbach & Janis [reported in Hovland (59)], deals with the effects of anxiety arousal associated with influence attempts. Three intensities of anxiety appeal were used with three groups in a "standard communication" concerning proper dental hygiene: a lurid description, with pictures; the same rules, in factual language, with milder illustrative pictures; and simple presentation of proper techniques, with no special anxiety-arousing material. The results indicate that strong anxiety arousal has detrimental effects in building sustained attitudes, as inferred from questionnaire responses concerning current practices, one week later. Findings are consistent with the authors' hypothesis:

The more strongly anxiety is aroused about the dangerous consequences of failure to adhere to recommendations, the greater the motivation to accept competing ideas which reduce anxiety by minimizing the importance of threat. Thus when anxiety is so strongly aroused and is not sufficiently reduced by reassuring recommendations as to what can be done, people may learn to ignore the threat.

Correlates of ethnocentrism and prejudice.—Several studies of personality correlates of ethnocentrism or prejudice have appeared during the year, most of them concerned with one or another aspect of the theory outlined by Adorno *et al.* in *The Authoritarian Personality* (1). In one of these, Campbell & McCandless (15) present a replication of some of the original investigations. Using as subjects a stratified sample of the nonminority members of a western college, they demonstrate once more the highly general nature of prejudice: intercorrelations among five subparts of a Xenophobia scale, as well as those among total scores of prejudice toward five different minority groups, are consistently high. The F-scale, presumed to measure personality predispositions toward prejudice rather than prejudice itself, shows consistent correlations around .5 with all of the preceding scores.

Assuming that, as the California studies indicate, there tend to be personality correlates of ethnic prejudice on the part of American subjects whose community norms do not particularly "prescribe" prejudice, the question arises whether this is also the case where subcultural norms call for "prejudice." Christie & Garcia (27) provide data which suggest a very plausible answer to this question. Responses from students in a southern university were compared with those of California students to the California Public Opinion Scale, and to various indices of "authoritarian personality."

Results indicate clearly that ideology with regard to minority groups shows about the same personality correlates among both groups, within the sanctioned frame of reference of each. The southern students, as a group, accepted more F-scale items, but essentially the same personality differences between high and low F-scorers appeared in both groups. No other differences—in social class, child rearing, urban-rural background, etc.—were found. Background data are presented from which the authors conclude that the southern students' environment had been "characterized by a narrower range of ideology which was fairly conservative in nature. Exposure to this relative paucity of ideological stimulation," rather than differential child-rearing practices, is believed to be the distinguishing variable.

Ethnic attitudes of southern adults have also been studied by Prothro (92), using generalized Remmers scales and Bogardus social distance scores to measure distinctive attitudes toward separate ethnic groups. For the 383 subjects as a whole, there was a correlation of .49 between attitudes toward Negroes and toward Jews, but the scatter diagram showed interesting differences among their subjects. Nearly all subjects favorable to Negroes were also favorable to Jews; a considerable number of subjects were unfavorable to Negroes but not to Jews; while subjects in a third category were unfavorable to both. These results are entirely consistent with those reported in the preceding study; one would expect "authoritarian personality" characteristics to be common among the third of these groups of subjects but not among the first, nor necessarily among the second.

Some personality correlates of "social intolerance" have been reported in a series of studies by Gough (48). A wide battery of scales and inventories was administered to several hundred high school seniors concerning whom many forms of personal data were also obtained. By intercorrelations and by contrasts of subjects extremely high and extremely low on the Levinson-Sanford scale of anti-Semitism, a pattern of personality correlates emerged. Of particular interest is the author's attempt to develop a scale of anti-Semitism which included no reference to Jews, through successive refinements of Minnesota Multiphasic Personality Inventory items repeatedly shown to correlate with the Levinson-Sanford criterion. The resulting "Pr scale" correlates about $.45 \pm .06$ both with the criterion and with the California E-F scale. The author's summary of the characteristics revealed by this scale is as follows:

The overall picture . . . is one of a harassed, tormented, resentful, peevish, querulous, constricted, disillusioned, embittered, distrustful, rancorous, apprehensive and somewhat bewildered person. . . . All of the tendencies here mentioned are socially isolating in various degrees and would be expected to interfere with efficiency of social interaction and response.

The main stress in these articles is on the "network of attitudes and beliefs into which the specified ethnic opinions are characteristically integrated," this network being "in tune with" the personality.

Another psychological variable reported to be correlated with ethno-

centrism is that of tendency to modify memories. Using a method of repeated reproductions, Fisher (42) found that frequency of "breaking the gestalt" or "altering the course of Pragnanz" is inversely related to ethnocentrism. This variable is presumably a special case of the rigidity which the theories of Adorno *et al.* have postulated as a determinant of ethnocentrism.

Cognitive processes.—Cognitive processes underlying attitude formation have been studied in an ingenious experiment by Galloway (46), repeated with slight modifications by Wyatt & Campbell (116). The experiment was designed to test the hypothesized "reduction in adequate perception due to previous unverified hypothesizing or guessing," and is presumed to provide an analogue for the formation of stereotypes. Responses of student subjects who viewed each of 12 lantern slides in increasing stages of clearness (beginning with an extremely blurred presentation) were compared with those of other subjects who saw the same slides only at one of the intermediate stages of clearness. Differences were highly significant; in both experiments, there was greater accuracy for the isolated presentation at a given degree of clearness than for the corresponding serial presentation, preceded by "guessing" under more ambiguous conditions. The later investigators suggest that "in social problem solving, experience per se without reality testing or verification may often be a liability, bringing reduced objectivity."

Factor analysis of social attitudes.—A small stream of factor analysis studies of social attitudes continues, with most attention being paid to the dimension of conservatism-radicalism. Lorr (81) concludes that at least two independent social attitudes underlie this dimension: the first he describes as political nationalism plus economic conservatism versus internationalism and economic socialism, and the second "may be considered to be bias in favor of individual freedom from social regulation and control as contrasted with that favoring control by social codes or governmental organization." He suggests the labels of individualistic-paternalistic and capitalistic-communistic for these continua. Sanai (96), using Burt's general factor method of simple summation, attempted to distinguish a general factor of conservatism-radicalism appearing in attitudes on religious, political, and "social" questions from other special factors. Analysis of responses of heterogeneous groups of London students revealed a general factor, labeled "alteration-preservationism," together with three others which were statistically significant; the game of "naming the factor" leads to the tentative labels of "religious vs. political-social alterationism," "authoritarianism vs. freedom from authority," and "social evolution vs. communism." These and other such studies—e.g., Eysenck (35)—find general factors, though the exact nature of the factors varies with that of the several scales from which they are derived.

ATTITUDES AS RELATED TO GROUP MEMBERSHIP VARIABLES

Judging by the past year, there is an increasing proportion of attitude studies in which some or even primary attention is paid to group variables.

This is quite as true of experimental as of survey investigations. With specific reference to attempts to create attitude change, Cartwright (19) outlines some considerations which are compelling social psychologists to formulate this problem in a group membership context. Eight principles, some dealing with groups as media and some with groups as targets of change, are presented. (E.g., a group's influence is limited by and is a function of its attractiveness to members; the effectiveness of a given influence is a function of its relevance to the basis of attraction to the group; the greater the shared perception by members of need for change, the greater the likelihood of change.) Experimental support is presented for several of these propositions. The article provides a concise statement of the theory from which a good many experiments on attitude change have been designed, and its point of view provides a suitable introduction to the studies referred to in the remainder of this section.

Group decision.—In one of a series of studies of group influences upon social attitudes at the University of Michigan, Bennett (9) succeeded in controlling various factors hypothetically related to attitude change following "group decision." Lewin (80) apparently believed that group discussion was a necessary though not a sufficient condition for reaching the group decisions which he and his associates repeatedly found so effective in inducing changes of attitude. By using subjects from 32 sections of an introductory psychology class, Mrs. Bennett was able to control the variables of presence or absence of discussion, of public versus private commitment (to volunteer for an unspecified psychological experiment), decision versus no decision, and perception of near-consensus on the part of other group members in volunteering. The first two of these were found not to be related to actual behavior in coming to the laboratory as a voluntary subject. In groups which were asked to make a decision one way or the other, whether publicly or anonymously, significantly larger proportions of subjects carried out the relevant action. And subjects who believed (usually more or less correctly) that large majorities of their groups had decided to volunteer were more likely than other subjects to take the action. In groups where decision was requested, and where most subjects perceived near-unanimity of willingness to volunteer, the degree of attitude change (as measured by actual behavior) approximated that reported by Lewin and his associates. The author's conclusion is that the function of group discussion is to facilitate decision and/or perception of consensus, and that any other procedure (including lectures) which performs these functions can be equally effective. (See also p. 11.)

Reference groups.—A distinction between two uses of the term "reference group" is noted by Kelley (74). It is sometimes used to refer to the fact that judgments are made in terms of one or more properties of a specified group, as a standard of comparison. The same term has also been employed to refer to presumed influence upon an individual of norms which he attributes to a group. He proposes to distinguish these two uses by the terms "comparative" and "normative," respectively. In a recent study of the latter type, Charters

& Newcomb (25), having previously identified the religious affiliation of the students in a very large class of students, assigned Catholic students to meet in one room "to carry out an experiment," while Evangelical Protestants, Jews, and a randomly selected group of women students simultaneously met in separate rooms for the same purpose. Subjects were immediately told the basis of their selective assignments, and after a brief and highly general discussion of the significance of membership in their respective groups, all subjects responded to the same attitude questionnaire. Other subjects representing the same groups were randomly assigned to a single control group, and responded to the same items without preliminary explanation of any kind. The hypothesis—that by thus increasing salience of membership in the experimental groups, their normative influence would be greater than upon comparable control groups—was confirmed for Catholic subjects only, and certain qualifications of the original hypothesis are proposed. The findings for Catholics are interpreted in terms of conflicting influences from church and university as normative reference groups. An item analysis is offered to show that on items most and least relevant to church norms the experimental subjects show most and least divergence, respectively, from university norms.

In a somewhat similar study Kelley & Volkart (75) studied the attitudes of 145 Boy Scouts in 12 urban troops toward camping and woodcraft activities, both before and after they heard a speech criticizing Scout emphasis upon such activities and recommending substitute activities more relevant to city life. Following the speech, half of the subjects expressed their attitudes under public and half under private conditions. Information was also obtained concerning each subject's valuation of troop membership. (A preliminary study had shown that Scout norms were "pro-camping"; a sample of Scouts were considerably more favorable to such activities than a comparable sample of non-Scouts.) The influence of the speech was found to be greater under public than under private conditions, a finding consistent with the basic proposition that the effects of an influence upon attitudes are a function of the norms of the reference group involved.

Other findings supported the hypothesis that attitude change is inversely related to valuation of membership in the group to a greater degree under private conditions than under public conditions. These and the other findings under the public conditions can probably be understood, as the authors suggest, in terms of the assumption that adult norms were particularly salient under public conditions (the speech was made with Scoutmasters' approval, etc.). If so, the experiment may be considered a study of the influence of conflicting reference groups. In any case the findings make it clear that resistance to attitude change is a function of valuation of membership in a group whose norms are in conflict with the influence to change.

Attitudes and social behaviors of Jews of differing national backgrounds living in Melbourne have been studied by Emery & Katz (34) in terms of the reference group concept. Various kinds of evidence, collected in inter-

views with 54 Jewish respondents as part of a sample survey, suggest that Jews born in eastern Europe, in western Europe, and in Australia tend to orient themselves toward quite different groups. As judged by reported memberships and preferences, their significant reference groups are as follows: for the Australian-born, the Australian Gentile majority; for the eastern-born, the Jewish minority; for the western-born (relatively new arrivals, and somewhat marginal) neither of these but rather the country of national origin. The authors point to some probable determinants of selections of reference groups, and to their functional value, either defensively or in relation to status aspirations. They note, furthermore, certain consequences of reference group selection—or at least correlates which are most parsimoniously accounted for in such terms. For example, amount of anti-Semitism perceived is not related to amount of own suffering from it, but rather to point of view in judging it: thus the western-born perceive relatively little of it, as judged by their recent west-European experiences; the Australian-born, for whom it is a barrier, and the eastern-born, for whom it is a threat, perceive a good deal more of it. For similar reasons the Australian-born almost unanimously attribute partial blame for anti-Semitism to Jews, while very few of the eastern-born do so. Unfortunately the numbers of cases upon which these differences are based are small.

There have, of course, been many studies of political attitudes as related to political party membership and/or preference. A recent study by Belknap & Campbell (8) is one of the few of these which makes any attempt to discover whether stated party preferences are associated with any of the commonly assumed properties of group membership. One such property is the common belief that attitudes toward political issues are shared with other group members. Measures of perceived consensus were therefore obtained by asking, in connection with certain issues, how many of their own preferred political party (and of other groups) they saw as agreeing with them. On the issue of the Truman-MacArthur controversy, for example, 85 per cent of all self-styled Democrats in a national sample who expressed an opinion believed that other Democrats agreed with them; the corresponding figure for Republicans was 90 per cent. Among Democrats with opinions, 61 per cent saw Republicans as disagreeing with them; the percentage for Republicans was the same. Even among the minority of Democrats supporting MacArthur (42 per cent as against 47 per cent supporting Truman) in the summer of 1951, two-thirds perceived other Democrats in agreement with themselves. Such findings are consistent with earlier ones, e.g., Centers (23), Chowdhry (26), Newcomb (88), Steiner (107), that perceived consensus with one's own position tends to be associated with positively valued group membership.

Another significant finding has to do with levels of information. Respondents categorized as of high, medium, and low levels of information on American foreign policy do not differ in attitudes toward those policies. But when these categories are subdivided in terms of party preference, differences

appear, particularly among the better informed. On every one of eight issues, well-informed Republicans and Democrats differ more in attitude than do other party adherents, though all differences are not significant. These findings are consistent with earlier ones [Newcomb (87)] which were interpreted as pointing to the interdependence of three variables: concern over an issue, membership in a group which prescribes either favorable or unfavorable attitude toward it, and relatively high information concerning it.

An interesting attempt to assess the political influence of several membership and reference groups is reported by Freeman & Showel (45). In connection with a pre-election survey, respondents were asked about their membership and activity in 35 organizations, and about their willingness to take advice from any of them concerning election issues. Responses are analyzed separately for members and nonmembers of the 13 organizations most frequently mentioned—e.g., veterans' organizations, business bureaus, parent-teachers associations, political parties and labor unions. Very large majorities—100 per cent in several cases—of members of each of the 13 organizations reported willingness to take such advice, but the net "influence scores" of about half of the 13 were negative. (There is, however, no evidence that organizations having negative influence scores functioned as negative reference groups, since a negative score means "not likely to take advice" rather than "likely to be influenced in the opposite direction.") Both the Republican and Democratic parties received negative scores from "nonmembers." A correlation of $-.35$ suggests that total influence (from members and nonmembers) is inversely related to size of organization.

Social class.—Many attitudes have been found to vary with social class status and/or affiliation. Conceptually, at least, these two indices of class membership are quite different, so that a comparison of the two methods of ordering individuals to a social class continuum should prove illuminating. Centers (22), following up his earlier study (23), has provided such data. Each of 100 respondents in Los Angeles was first scored by Warner's method (112) of "evaluated participation" (known to correlate highly with his index of status characteristics) and also asked to choose one of the following class affiliations: upper, middle, working, lower. Findings indicate that both upper- and middle-class assignments are about the same by both methods, that most working-class affiliates have lower-class participation scores, and that lower-class affiliates appear at the very bottom of the distribution of participation scores. All this suggests (as Warner has claimed) that most respondents are able to place themselves fairly accurately, particularly if they are allowed to substitute "working" for the less welcome "lower" affiliation. It still seems to be true, however, that the discrepancies between class assignment by ascription and by self-affiliation are frequent enough so that they can not be considered interchangeable measures.

Sims (104) proposes a new technique for measuring social class identification, bringing to bear two criticisms of Centers's procedure: valid re-

sponses presuppose particularly good rapport; and responses are crudely categorized, rather than falling on a continuum. Sims had 646 respondents of heterogeneous backgrounds indicate whether people in 42 different occupations belonged to a higher, a lower, or the same class, compared to themselves. A simple qualitative index, obtained from the ratio of these three kinds of responses, was closely related to own statements of class position. The author feels that the method has the virtue of being concealed; only one of 69 students suspected that it was a measure of class position.

Hammond (49), in a partial report of a "community study" in Australia, relates objective (occupational) class data to subjective "frameworks" as inferred from responses to the "open" questions, "A society is made up of groups of people. To what part of society do you belong. . . . What would you say are the main groups in Australian society?" Nearly all answers were in terms of class notions, but to some this meant class conflict, to some hierarchical "levels of living," and to others broad interest groups hierarchically differentiated. These three "standpoints" are shown to be related to present occupation, to class origin, and to subjective status.

If, as commonly supposed, there are consequences for attitudes and behavior of status strivings with relation to various groups, then the problem of the determinants of status strivings becomes an important one. An attempt has been made by Fenchel *et al.* (38) to test an hypothesis of Benoit-Smullyan (10) concerning "status equilibration," to the effect that individuals tend to attain a common level in various hierarchies of reference. Using an adaptation of Hyman's status scale (62), the authors obtained responses by 72 students indicating both actual and desired standing in relation to each of five reference groups. Results, supporting the hypothesis, indicate a tendency for status strivings to be highest in relation to the group in which actual status was lowest; thus individually desired statuses in the several reference groups tended to reach a common high level. Three "devices" for achieving equilibrium in the status structure are distinguished: satisfaction with existing status, desire for more and desire for less than presently achieved status. The last of these was exhibited by more than 20 per cent of all ratings, and suggests the likelihood of negative reference group influence.

Perceived consensus.—Several studies from the University of Michigan (in addition to that of Bennett and that of Belknap & Campbell, above) have used as a variable "perceived consensus" (judgments of degree of agreement with respect to attitudes, information and/or cognitive structuring which are group relevant). As an intervening variable, it is hypothetically a function of opportunity to observe other group members and of autistic factors (e.g., desire to see oneself as like other group members); the individual's own attitudes, beliefs and cognitive structuring, in turn, are hypothetically functions of his perceived consensus. Earlier studies [cf. Centers (23); Newcomb (85); Travers (111)] have shown with great consistency that attributions of attitudes to other members of positively valued groups vary

closely with own attitude. A similar finding appears in a report of a national sample survey of attitudes toward big business by Fisher & Withey (41). Respondents who had already expressed their own opinions were asked to judge whether each of 10 "kinds of people" (e.g., rich people, labor union members) would be likely to agree among themselves in attitude toward big business, and how all or most of them would feel about the matter. Respondents were further asked to select the groups most likely to agree and to disagree with them. Rank order of perceived agreement with the 10 groups provides a better predictor of own attitude than does any other single variable. Demographic variables proved unrelated to attitude, as did level of information. According to the authors' summary,

Most respondents do not seem to adopt . . . a simple ideological approach to big business. Their attitudes hinge rather on perceived benefits to themselves or to groups with which they identify.

Steiner (107), using the same body of data, found that mean scores of attitude toward big business of those who believed themselves in agreement with each of the 10 reference groups, when arranged in order from most to least favorable, followed the predicted order very closely, "people who run big businesses," "Republicans," and "rich people" holding the first three ranks, and "labor union members," "poorer people," and "people who run small businesses" the last three. Differences between the first and last of these groups were significant at the .01 level. When each of these groups was further subdivided between those who did and did not say that some or all of their 10 closest friends would agree with them, the mean scores of the latter for the 10 reference groups become still further differentiated. Those who believe themselves in agreement both with closest friends and with "people who run small businesses" and/or "poorer people" and/or "labor union members" are extremely anti-big-business in attitude. Such findings could alternatively be accounted for on a "reality" basis (i.e., people tend to associate with others of similar attitudes) or on an "autistic" basis (i.e., people tend to attribute their own attitudes to their friends). Perhaps it is because both factors work in the same direction that indices of perceived consensus so often provide such a good index of attitudes.

Such findings, however, tell us nothing about degrees of pressure to conform, and individuals surely differ in this respect. Steiner, therefore, using heterogeneous groups of adults totaling 278 subjects, obtained questionnaire responses comparable to the foregoing and also responses designed to measure "perceived primary group conformity measures"—e.g., "If some or all of your 10 closest friends were to disagree with you about big business, how much would it matter to you? . . . to them?" Findings, analyzed in several ways, are in support of the general proposition that the degree of an individual's deviation from his perception of the consensus of an attractive group's attitude varies inversely with the strength of conformity pressures within that group.

Large organizations.—Two reports of comprehensive studies of social relations and attitudes in industrial firms have appeared during the year. The first of these, by Katz & Kahn (72), represents a distillation of findings in several large organizations each of which was studied over considerable periods of time by the Human Relations Program of the University of Michigan's Survey Research Center. Introducing their findings as "part of a long-range effort to discover the social-psychological determinants of organizational effectiveness," the authors note that

the group situation is not a mere multiplication of the single individual . . . [but] a matter of people interacting. . . . It is therefore a mistake of major proportions to take the individualistic model of reward and punishment from the laboratory (where reward can be manipulated directly in relation to behavioral outcome) . . . these studies argue strongly for the inclusion of social-organizational variables, or at the very least of perceptual and attitudinal factors which reflect the complexities or organizational structure.

Since industrial organizations are typically made up of a number of interlocking social systems, much of the analysis is done in terms of role assignments (see page 203). Data from several organizations are presented to show that there is "little evidence that differential perception or attitude toward the rewards furnished by the company is related to energy devoted to the work." Furthermore, in two large departments of office workers those who were more critical of the company placement system and rating system were not necessarily low producers—those in one department, in fact, tended to be high producers. ". . . critical attitudes are often found more often among the interested and informed people." The data point to "group belongingness" and "ego needs," rather than to reward systems set up by management to account for "differential effort" and productivity. Workers in high-producing groups tend to rate their groups above others in ability to get work done—even when objective information is not available to them. Further data are presented to show that satisfactions are closely related to opportunity for full use of skill, and that high productivity is associated with lesser rather than with greater closeness of supervision—pointing, perhaps, to needs of "self-determination."

A partially overlapping discussion of the relations of organizational variables to productivity in large organizations appears in a paper by Kahn & Morse (70). The conceptual relationship of productivity to morale is analyzed in terms of need strength, availability of alternative behaviors, opposing tensions set up by the behavior in question, and the ratio of need-satisfaction to required input of energy.

A group of situational factors, labeled "organizational climate," was found by Hariton (52), working with N. R. F. Maier, to be related to the degree to which foremen accepted and utilized their training in "human relations." In a large firm in two divisions of which foremen had received comprehensive training, he found a significant increase in employees' satisfaction with supervision in one division, but a significant decrease in the

other. Differences seemed to be related to the following characteristics of foremen which were more common in the first than in the second division: satisfaction with foremen's supervisors; receiving encouragement from supervisors to use training principles; expectation of benefiting from training; satisfaction with own jobs and promotion chances; relative youth. The conclusion is that the impact upon employee attitudes of training programs for foremen depends on the degree to which attitudes and practices of high levels of supervision are consistent with the content of the training program.

Pelz (90), working on a similar problem, also found that attitudes of employees could be predicted from attitudes and practices of supervisors in some work-units but not in others. He was attempting to test certain derivations from the basic postulate that acceptance of supervisors by employees is a function of their helping employees to achieve their goals. Break-downs by hierarchical and demographic categories yielded inconsistent results, but clear differences were found when relationships were calculated separately for the more and the less influential supervisors, as judged by self-reports of autonomy, of participation in higher level decisions, and by salary level. Thus the general finding is that employee satisfaction with supervisors is a direction function of the latter's helpfulness to employees in reaching their goals if and only if supervisors are relatively influential, or powerful.

PROCESSES OF INTERACTION AND COMMUNICATION⁴

Both conceptual and empirical contributions in this area are beginning to achieve a theoretical significance. Conspicuous among such attempts at theoretical integration is the volume edited, and in considerable part written, by Parsons & Shils, *Toward a General Theory of Action* (89). Presenting their general theory at a purely conceptual level, they distinguish three interdependent systems of action: personality, social system, and culture. The second of these "is organized around the problems inherent in or arising from social interaction." Pointing to the basic differences between orientation toward social and toward nonsocial objects, they stress "reciprocity or complementarity of expectations," which is dependent upon communication through a common system of symbols, as essential to an integrated system. Culture (the symbol system) provides the set of symbols which also function as a set of norms for action. Through interaction involving varying degrees of gratification, the motivations of interactors become integrated with the normative patterns, and reciprocal role-action is learned; roles thus serve as instruments for motive satisfaction, as shared symbols, and as normative behaviors. With great ingenuity and theoretical rigor, though without attention to the researchability of their propositions, the authors thus present

⁴ The following students have contributed to the preparation of materials for various parts of this section: S. Berlinsky, G. Briskin, S. Epstein, J. Kaufman, M. Monk, D. Pollie, J. L. Weiss.

a theory of interaction which is closely articulated with their theories of individual and of societal behavior.

As a general contribution to the theory of interaction, Sears' 1951 Presidential Address before the American Psychological Association (101) is significant—not because social variables are taken into account but rather because of his demonstration of how “the basic monadic unit of behavior” (stimulation-instrumental acts-environmental event-goal response) is “expandable into a dyadic one.” Drawing, as do Parsons & Shils, the distinction between social and nonsocial environmental events, he notes that “the factor responsible for maintaining stability of the dyadic unit . . . is the *expectancy* of the environmental event.” His paradigm for including two (or more) monadic sequences in a single system is derived from that used by Hull (61) for the anticipatory goal response. Sears makes no claim that such a conceptualization of relationships between people is adequate for a theory of interaction, but any such inclusive theory must of course include a psychological accounting of interdependent individual behaviors, such as he suggests.

Two general treatises on communication have appeared during the year under review. With primary attention to problems and data of psychiatry, Ruesch & Bateson (95) attempt a “unified theory of communication,” borrowing heavily from cybernetics and the theory of games, on the one hand, and from psychiatry and cultural anthropology, on the other. Properties of “networks” at four “levels of communication” (intrapersonal, two-person, group, cultural) are described. Perhaps the most characteristic emphasis of the volume is that upon self-correction: “the ability of an entity to predict events and also with the entity's ability to modify its action when these predictions are shown to be in error.” The second volume is a textbook by Miller (84) on language and communication. Relying heavily on Hullian behavior theory and upon information theory, he takes up successively phonetics, perception of speech, symbols, learning, verbal behavior, “words, sets and thoughts.” In a final chapter entitled “the social approach,” which draws especially upon the experiments of Bavelas, of Leavitt, of Heise & Miller (p. 199), and of Festinger and his associates (40), some of the demonstrated determinants and consequences of communications within small groups are presented. There are brief sections on large groups, mass media, and rumor. Miller makes an important distinction between primary and secondary information, and notes some of the functions of the latter, defined as “knowledge about who knows what,” which is made possible by “feedback” and self-correction. No other book known to this reviewer brings together in a psychological treatment such a wide range of data in this area.

Communication in small face-to-face groups.—A small but significant number of studies of the kinds reported in this chapter in previous volumes, especially those of Festinger, Bavelas, and their respective associates [see Katz (71); and Smith (105)], continues. Bavelas (7), in a brief summary of the logic of his studies in communication networks, presents new evidence

together with reviews of some previously published ones—e.g., by Leavitt (78). He also outlines an intriguing experiment on the relationship between communication patterns and the occurrence and utilization of insight. In a tentative generalization he notes that

in patterns with a high, localized centrality, organization evolves more quickly and is more stable, and errors in performance are less. At the same time, however, morale drops. It is conceivable that poor morale would, in the long run, affect stability and accuracy negatively.

If research of this kind can establish conditions under which efficiency does or does not improve at the expense of morale, it will have immense practical value.

In a study of the efficacy of "feedback" by Leavitt & Mueller (79), the transmitting and receiving of information making it possible for subjects to reconstruct several geometric patterns was observed under conditions of no feedback, partial, and free feedback. Results, under these specific conditions, are quite clear cut: feedback increases accuracy, increases the confidence of both sender and receiver in their degree of success, and takes additional time. Since feedback experience improves performance in later trials without feedback, it is concluded that "free feedback seems to permit the participants to learn a mutual language, which language once learned may obviate the necessity for further feedback." It was also found that some hostility was engendered in the no-feedback situation, but not with free feedback.

Heise & Miller (56) report an ingenious experiment in which all of the following were varied: communication possibilities within three-member groups (via telephone, each member being in a separate room), nature of task, and amount of "noise" introduced by the experimenter. In a task of reconstituting a set of words known only in part by any one subject, the efficiency of reaching solutions varied considerably among the communication nets: the greater the noise, the greater the importance of secondary information; the clearer each member's view of the group's pattern of information, the less the interference from noise. Findings were quite different, however, with a less mechanical problem; here noise was best resisted by the group in which the occupant of a central position was forced into "leadership." In another problem where individual initiative was more important than communication, no network was more resistant to noise than any other. As the authors point out, the effects of varying channels of communication depend upon the nature of the group's goal.

Bales *et al.* (5), using previously reported methods (4) of observing "interaction processes," have studied the formation of communication channels in groups of from three to ten while they were in the process of solving problems in their natural setting. The source, direction, number, and kinds of communications were tabulated on a matrix in rank order of number of communications originated by each subject. A significant correlation

was found between frequency of originating and of receiving communications. Frequent communicators characteristically offered information and opinions; infrequent ones made statements of agreement or disagreement, and requested information.

In a related study Bales & Strodtbeck (6) report a study of "phase movement" in the course of group problem solving. The phase hypothesis is based on the assumption that there are internal tendencies of interaction considered as a system distributed between persons and through time. A series of changes in the social-emotional relationships of the members tends to be set in motion by pressures arising initially from the demands of the external problem of the outer situation and tends to be expressed in overt interaction.

There is probably no group property more often referred to in recent discussions of group behavior than that of cohesiveness. Schachter *et al.* (99), defining this property strictly in terms of the attractiveness of the group for its members, in distinction from what they regard as looser definitions (e.g., "morale"), present experimental support for a proposition derived from the theory from which their own definition is drawn. Using simple devices for manipulating both cohesion (i.e., attractiveness) and of forces to "speed up" as opposed to forces to "slow down," they find "no necessary relationship between cohesiveness and high productivity. Group members will accept induction either to increase or decrease production," and highly cohesive groups can with equal readiness develop standards of either kind. This finding is contrasted with the contrary hypothesis—that high productivity is a function of high morale—which they believe stems from the other kind of definition of cohesiveness.

Some interesting correlates of frequency of interaction have been reported by Bovard. In two "group-centered" classroom groups verbal interaction among members was encouraged, while in two "leader-centered" groups such interaction was "politely but firmly held to a minimum, the leader intervening when any member-to-member exchange did spring up." These procedures were continued throughout an entire semester, and recorded observations showed enormous differences in frequencies of verbal interaction. Toward the end of the term the groups were compared in two respects. One concomitant of this differential procedure was a significantly higher rating of "interpersonal affect" (based on like-dislike ratings) in group-centered than in leader-centered classes (14). Particularly in the former groups, moreover, the group as a whole was rated a good deal higher than the average of the individual ratings. These groups also showed greater dispersions of affect ratings than did the leader-centered ones. The author attributes these differences to the differential frequency of interaction, which makes possible the correction of perceptual distortions of self and others. The two pairs of groups were also found to differ in shifts of judgments of the length of a rectangle following announcement of the mean judgments made by the group members a few minutes earlier (13). All groups tended to

shift toward the announced norm, but the shift was considerably greater on the part of the group-centered than on the part of the other classes.

Some interesting hypotheses concerning opportunity for interaction, and derivations therefrom, appear in an experiment by Hare (51); He studied degrees of agreement, before and after discussion in groups of five and of twelve Boy Scouts concerning the importance of 10 pieces of camping equipment. He found greater consensus following discussion in small than in large groups. In the smaller groups there was a much larger tendency, in individual evaluations made following the group decision, to report that the group evaluation was "correct"; this difference appears to be related to differential opportunities for participation. "If an individual has a chance to present his ideas, even if they are not accepted, he is generally satisfied with the results of the discussion." The author believes that the larger groups also limit interaction by increasing members' feelings that their opinions are not worth presenting to the group. There has been very little evidence of such direct nature as this in support of the proposition that consensus is a function of opportunity for communication.

In a study stressing cognitive aspects of interaction, Horowitz, Lyons & Perlmutter (58) tested some propositions suggested by Heider (55). According to this theory, the perception of a negatively valued act by a positively valued person (or vice versa) results in disequilibrium, the stress of which is likely to be counteracted by re-evaluating either the act or the actor. Using members of continuing discussion groups as subjects, they obtained evaluations of people and of recently observed events occurring in the groups. Each member was also asked to indicate how other members would have felt about these events, and records were taken of all verbal interactions. Results are in agreement with the predicted differences in ratings assigned to acts associated with positively and negatively valent people. There was also a tendency for subjects to see liked persons as agreeing and disliked persons as disagreeing with their own judgments of an individual's acts (as distinguished from valuing the individual himself). There was thus a tendency toward intrapersonal harmony of judgments of persons, of their acts, and of their judgments of others' acts. "Objectivity tends to be lost in the interest of psychological harmony."

*Institutional and community therapy.*⁶—Contact and communication have long been assumed to be important determinants of intergroup attitudes, but only rarely have changes in both been studied following a deliberate attempt to create them. Festinger & Kelley (39) report such an experiment. A program of community activities was introduced in a war-built housing project whose residents were characterized by social isolation from the immediately adjacent city, by hostility and few contacts among themselves,

⁶ It has been necessary to omit a section, prepared for inclusion at this point, on interactional aspects of group therapy, because of space demands. This topic is treated from a somewhat different point of view in Chapter 14.

and by exaggerated perceptions of rejection by townspeople. Beginning with the hypothesis of autistic hostility (86), they sought to discover whether hostility could be lessened by increasing contacts (and hence, presumably, communication) among project residents. Although the average number of contacts within the project was nearly doubled, the over-all amount of hostility was slightly increased: decreased hostility among project residents was largely limited to those who already had favorable attitudes toward community activities. As to relations with town residents, there was an over-all slight decrease in perception that townspeople regarded project people as a low-status group, but also a slight reduction in contacts between town and project residents. The most clear-cut finding was a polarizing effect—i.e., that there were increasing differences between those favorable and those unfavorable to community activities. "The broad effects of the change program were, for practical purposes, insignificant . . . in some respects for the better, in others for the worse."

Such findings (together with many others) lead to the following qualification of the autistic hostility hypothesis.

Persons with hostile (or favorable) attitudes toward a particular group will tend to establish and maintain contact with others who share their attitudes, consequently these attitudes will tend to be maintained and reinforced.

There was, as hypothesized, a reduction in the amount of contact on the part of the townspeople with the project people as the attitudes of the latter became more hostile, but there was a lessening of hostility on the part of both project and town residents who made new contacts under conditions which indicated some favorable attitudinal predisposition.

The 1951 Kurt Lewin Memorial Award Lecture, by Wilson of the Tavistock Institute of Human Relations in London (115), is devoted to "examples of mechanisms which operate at the level of both the individual and the group, but without recognition." For example, a 30 per cent increase in productivity of coal miners is described as the result of a change designed to modify group structure, and with it a host of attitudes. The standard, and still dominant, system

may possess an unrecognized function: that of permitting the carrying out of a production task in a way which also provides for the absorption of reciprocal hostility between management and workers. . . . At the same time this hostility is fed and maintained through the frustrations generated by the system. The common control by this work system, which forced isolation on the job, permitted common allegiance to the worker-management split, and common acceptance of the existence at the coal-face of a leaderless work group with groupless leaders.

The lecture concludes with the theme of "the prevalence and complexity of unrecognized mechanisms in group life, and their importance in maintaining quasi-stationary equilibria which resist change."

Interaction processes, with especial attention to authority relations, are the subject of a detailed case history by Jaques (68) of the changes and resist-

ances to change which accompanied a three-year consultative relationship between a London factory and the Tavistock Institute of Human Relations. Barriers to communication are described in some detail, both at the level of personal fears and wishes and at the level of formal factory organization. Of particular importance is "role confusion," since accurate communication presupposes common understandings of who is communicating to whom in what capacities. Role confusion was prevalent in authority relationships because of general failure to understand the nature of "the sanctioning process" by which consent is given to the occupant of a position by peers, superiors and subordinates to exercise specified kinds of authority within a specified range. "Role clarification" was the process by which attempts were made to establish common understandings of both upper and lower limits of authority, and of obstacles to its exercise—obstacles of both personal and institutional nature. Resistance stemming from "unrecognized motives" is especially stressed, and the term "collusion" is applied to instances of unwitting conspiracy. For instance, both management and workers, for very different sets of reasons hardly known to themselves and much less communicated about, behaved in ways which could be accounted for only by assuming motivation to continue the traditional management-worker split. Thus inefficient organizational practices are maintained: they serve simultaneously to satisfy unrecognized motives and to prevent the communicative processes by which they might be corrected.

In a very different sort of volume, Maier (83) presents detailed instructions for a wide variety of suggested procedures for changing supervisory practices in industry, together with a few investigations and the outlines of the theoretical position upon which they are based. Recommended procedures—all consistent with the general theory of "democratic supervision"—include discussion methods, role playing, and group decision, with many variants of each. Several previously reported experimental findings are reported—e.g., to the effect that "the democratic leadership technique is a useful procedure not only for obtaining acceptance and cooperation but also for improving solution quality" (82). The volume is not presented as an essay in interaction theory, but more nearly resembles a handbook on supervisory practice toward the objective of more desirable interpersonal relations. [See also Hariton (52)].

Role analyses of interaction.—Role-taking is often viewed as the basic process in social interaction, but role behavior has not often been studied in relation to specific variables. Hypnotically induced age-regression has been used for this purpose by Sarbin & Farberow (98). The general hypothesis is that the behavior of an adult under hypnotic suggestion that he is a child of a given age is determined by his perception of that age-role, which in turn is determined by his present self-structure. Six students were observed under hypnosis; Rorschach, Draw-A-Man, graphological and interview responses were obtained at the suggested ages of 3, 6, 13, and 18. Analyses are primarily in terms of two variables: stability of self-organization (derived mainly from

Rorschach responses) and flexibility of role-perception. The behavior of two contrasting subjects, presented in some detail, seems to be interpretable by assuming that one is characterized by inflexible role-perceptions and unstable self-perceptions, and the other by their opposites; other combinations are also noted. A general theory is outlined to the effect that role-takers are motivated to validate their occupancy of a position, and that variations in the social validity of role-taking are a function of validity in perceiving self-other relations, of role-taking aptitude, and of the existent organization of the self. Part of Sarbin's role-and-self theory is further outlined elsewhere (97). Hypnosis seems to be a particularly good device for creating experimental variations in role behavior, and thereby for contributing to a general theory of social interaction.

The concept of role is particularly useful for studying group-structure determinants of social interaction. Jacobson, Charters & Lieberman (65) present a report describing some role-determinants of attitudes and behavior in an automobile factory. For example, responses of workers, foremen, and shop stewards were coded as revealing expectations of primarily active or primarily passive behavior on the part of stewards; the modal expectation on the part of foremen was passivity, while that of workers and stewards was activity. Findings provide support for the proposition that "ease of interpersonal relations" between foremen and stewards is a function of agreement between them on the steward's role. These and other similar findings reinforce the theoretical position according to which adequate communication presupposes shared understandings or "a mutual field." With particular attention to multiple and conflicting role demands, the authors present partial findings which lead to the proposition that effectiveness of supervision is a function of ability and motivation to meet role expectations of relevant peers, superiors, and subordinates. [See also Jacobson (64); and Charters (24).]

A study of role relationships of mothers and children is reported by Cass (20), the significance of which extends beyond the problem of delinquency toward which the study was directed. Subjects were 21 "seriously maladjusted" adolescent delinquents, individually matched with "well-adjusted" children. The major interaction variables, "awareness, identification, and projection," were derived from a check-list questionnaire responded to by subjects and also by their mothers who, in addition, filled out the same questionnaire "about her child." Awareness is measured by the mother's success in predicting the child's responses, identification by the actual similarity of own responses, and projection by assignment by the mother to her child of traits checked for herself but not checked by the child. Parental conflict and control are also measured from children's responses. Hypothetical predictions are supported by these findings: mothers of delinquents show less awareness of their children's preferences and fears than do other mothers; delinquents report greater parental control than do nondelinquents; conflict scores are higher for delinquents than for nondelinquents. These and other

findings are interpreted in terms of "social learning theory": assuming awareness by the parent to be satisfying to the child, and control to be frustrating, "the effects of the opposing forces of positive reinforcement through awareness and the negative reinforcement of control generalize to the area of social adjustment." In terms of interaction theory, the awareness variable seems conceptually related to the "mutual field" (Asch), to "perceived consensus," and to "reciprocity or complementarity of expectations" (Parsons & Shils), already discussed.

That different role assignments within the same group may be related to differential productivity is demonstrated in a laboratory experiment by Pepitone (91). She assigned to pairs of college girls a task involving two functional roles: planning a design and sorting pieces. Instructions were so varied as to lead some pairs to believe that the two tasks were of equal importance, and others to consider them very unequal. Productivity was found to be significantly related to the degree of importance assigned. Since other variables were held constant, it is concluded that forces toward productivity are a function of perceived importance of one's own position, under these conditions.

The leader's role.—Two more of the series of studies by Carter and his associates on leadership and group behavior have appeared during the year. In one of these (17) it was found that, while the behavior of groups differs in regard to leader-follower behavior depending on the kind of task (reasoning, mechanical assembly, discussion), some general leader ability appears in all of the particular tasks used. With smaller groups the traits rated in this study did not intercorrelate as highly as with larger groups. The authors believe this is because more of the basic abilities of the individual can be revealed in a small group—less specialized behavior is shown. There was also some evidence that these experimental ratings correlate with actual behavior of Navy reserve officers on board ship.

The other study in this series is an investigation (18) of the behavior of group leaders in several kinds of tasks and in two kinds of leadership situations—one in which the leaders were appointed and one in which the leaders emerged as a result of interaction within the group. Some kinds of leader behavior were apparently a function of the task, regardless of the situation; others appear to be a function of whether the leader emerged or was appointed; appointed leaders tended to be democratic and emergent leaders more authoritarian. The authors explain this unexpected result by saying that

the appointed leader conceives his role as one of coordinator rather than a director or controller of the group's activities, whereas the emergent leaders had to establish their positions by being forceful and strongly supporting their own proposals in competition with other potential leaders.

Berkowitz (11), in another report of the University of Michigan Conference Research Project, has investigated the effect upon conference outcome

of the performance of the chairman's functions by members other than the chairman. Seventy-two conferences of groups in business, industry, and government were observed. Interviews, questionnaires, ratings by observers, and ratings by participants yielded measures of group conflict, cohesiveness, productivity, and satisfaction of participants with the conference. It was found that conflict tended to increase and satisfaction and cohesion to decrease with greater amounts of leadership sharing, even when the nonleaders supported the chairman in their performance of leadership functions. These findings were characteristic of all of a widely varying set of groups, except those facing urgent problems.

Chowdhry & Newcomb (26) attempted to test the hypothesis that "chosen leaders of a group are superior to nonleaders in estimating group opinion on issues of high relevance to that group, but not on issues of little relevance." Their assumption was that this hypothesis (which was in general confirmed) would support a theory of leadership which presupposes interaction among group members who share interests and standards, as opposed to a theory based primarily on individual differences, skills, and capacities. They used four natural, face-to-face groups organized about certain interest patterns and permitting the emergence of leadership and isolation. Three attitude questionnaires were administered to each group, the content presumably representing three different levels of relevance to the group's common interests. Sociometric data permitted classification of members as leaders, nonleaders, and isolates. Each member indicated his degree of agreement with each statement, and the percentage of the group he believed agreed with it. Results showed leaders to be superior to nonleaders and isolates, and nonleaders to be generally superior to isolates, on relevant issues. There were few differences on nonrelevant issues, and inconsistent results on intermediate issues.

These results seem to contradict those reported by Hites & Campbell (57), who found leaders in college fraternities not to be superior to nonleaders in estimating group opinion.⁶ It is possible that one of the differences between the two investigations was that of familiarity with the issues responded to. Though full data are not reported, it seems likely that the content of the Hites-Campbell questionnaire was either completely nonrelevant or so thoroughly familiar that there was little dispersion of response. In neither study was an objective measure of relevance used. In any case the Hites-Campbell findings suggest a revision of the Chowdhry-Newcomb hypothesis to the effect that superiority of chosen leaders over nonleaders in estimating group opinion is a curvilinear function of relevance—i.e., superiority is greatest when issues are sufficiently relevant to the group's common interests to pro-

⁶ According to unpublished data from subsequent studies, independently conducted, Professor Campbell and the present reviewer have each confirmed the results of the earlier study from his own laboratory. It is hoped that future research along the lines indicated herewith will succeed in controlling the factors responsible for these contradictory findings.

duce interaction from which attitudes may be judged, but not so central as to produce interaction from which uniform judgments are made by leaders and nonleaders alike.

Cattell (21) has defined leadership in terms of "syntality," a term previously suggested to define for a group what personality defines for the individual. A leader, he suggests, is a person who has influence upon group syntality, and changes thus produced provide the measure of leadership. In describing leadership, other concepts are also necessary—e.g., synergy, which is derived from the attitudes toward the group of all its members. In any group a certain amount of synergy is spent on maintaining group cohesion (intrinsic synergy) and the remainder goes to attaining the goals of the group (effective synergy). In helping to choose a goal, the leader can effect a change in the individual member's erg contribution to the group, i.e., if the goal is satisfactory, more individual energy and hence synergy will be forthcoming. In attaining a goal the leader can effect changes in the intrinsic-effective ratio of synergy. These changes can be investigated by factoring out at different times syntal characteristics from the correlation of selected behavioral variables used in examining groups. No empirical evidence is presented.

Two samples of investigations of individual characteristics of college student leaders may be mentioned. Cobb (28), applying the Goodenough Speed of Association test to women subjects (50 leaders and 50 nonleaders), found no significant differences in masculinity-femininity, significantly higher scores in leadership and barely significant differences in commonality—i.e., leaders tended more than others to make nonidiosyncratic responses. In many respects Goodenough's earlier findings (47) were corroborated. Evidence is presented suggesting that these leaders were more objective, more emotionally controlled, broader in their world views, and interested in more things beyond their personal lives than nonleaders. Williamson & Hoyt (114), in a somewhat similar study using the Minnesota Multiphasic Personality Inventory, find that "political" leaders (especially those of liberal-to-radical cast) characteristically show "unstable" or "neurotic" tendencies which distinguish them from fraternity and sorority leaders, who tend to be "just students."

A NOTE ON METHODOLOGICAL CONTRIBUTIONS

Space limitations have made it impossible to cover the considerable number of methodological contributions which have appeared during the year. Readers interested in sampling problems in survey research will find reports of experience in Durant (33), of a comparison of quota and area-probability sampling in Haner & Meier (50), and of a study of stratification procedures in Showel (103). Dodd (31) suggests a list of principles for improving poll predictions by accounting explicitly for certain important variables.

Publications on problems of interviewing include reports on the effects

of interviewer characteristics by Feldman *et al.* (37) and by Sheatsley (102), both from the National Opinion Research Center; an analysis of problems encountered by unskilled interviewers by Knower (76); a report of a study by Stock & Hochstim (109) in which interviewer variability is shown to account for "a sizable part of the total statistical error"; an inclusive statement by Hyman (63); and a report by Wedell & Smith (113) of discrepancies between interviewer ratings and respondents' self-reports of the same attitude. Both Stember (108) and Thayer (110) report considerable inconsistency of response to interviews; the latter made use of a panel of respondents in Japan. Of particular interest, also, is an investigation by Kahn (69) in which comparable interview responses and questionnaire responses were obtained from the same industrial employees. Contrary to the common assumption that the former are invariably more dependable, Kahn found that there was less nonresponse and fewer extreme responses to the questionnaire, and that differences revealed by the two methods were quite consistently greater for threat-oriented questions. The conclusions seem inescapable that anonymity may reduce forces not to communicate, and that "unwillingness to communicate causes measurable distortions of response."

Discussions of "indirect" measures of opinions and attitudes include a general treatise on the use of such methods in surveys, by Cobliner (29); the description of a "direction of perception" technique (based on error-choice methods of Hammond and Wechsler), by Bernberg (12), and a description, with preliminary findings, of a pictorial doll-play technique for the study of children's intergroup attitudes, by Hartley & Schwartz (54).

The following represent contributions of interest on other methodological problems: a statistic proposed by Schultz (100) for evaluating percentage agreement among judges of qualitative materials—e.g., content analysis—which takes into account the probability that judges are using established criteria; a report by Freeman & Haer (44) of a study of unidimensionality of attitude (in Guttman's sense) in which items yielding reproducibility for selected subjects failed to correlate with other items, presumably from the same universe, as responded to by the same subjects; the report by Canter (16) of an attempt, following an earlier paper by Solomon (106), to solve some problems involved in the use of multiple control groups in human relations studies; and three contributions to a symposium, respectively on probability methods by Reichenbach (94), on mathematical models by Arrow (2), and on qualitative measurement by Lazarsfeld & Barton (77).

Finally, the appearance of the two-volume *Research Methods in Social Relations*, edited by Jahoda, Deutsch & Cook (66) for the Society for the Psychological Study of Social Issues, is a social-psychological event of importance. Most of Volume I, on "basic processes," is written by the editors. It carries the reader from the selection and formulation of a research problem through research design, methods of data collection, analysis and interpretation to the implication of findings for theory and for practical application. Volume II, together with three appendices to Volume I, deals with more

specialized topics, each chapter by a different contributor: constructing questionnaires and schedules (Bureau of Applied Social Research, Columbia University), interviewing (P. B. Sheatsley), field work methods (W. F. Whyte), observation of small groups (A. Zander), content analysis (D. V. McGranahan), sociometric analysis (C. H. Proctor and C. P. Loomis), panel studies (M. Rosenberg, W. Thielens, P. F. Lazarsfeld), community self-surveys (M. H. Wormser and C. Sellitz), sample design (P. J. McCarthy), scaling theory and methods (S. A. Stouffer), and "assumptions underlying the use of statistical techniques" (L. Festinger), in addition to a special appendix on the measurement of discrimination and prejudice (I. Chein). Few if any of the procedures here described and critically reviewed are exclusively the province of social psychology, and yet many of them have been pioneered or in some special way developed by people who describe themselves as social psychologists. At any rate the sophisticated researcher in this field of scientific endeavor must be able to use several (and probably to understand all) of these special procedures. And no matter what his existing state of sophistication, the social psychological researcher will have something to learn from the detailed and critical outlining of the several steps which all methods have in common.

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INDUSTRIAL PSYCHOLOGY¹

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GENERAL

There was a larger volume of publications in the area of human relations during the past year, and there promises to be a continued rise in interest in this area. The specific topics of greatest popularity were (a) the effectiveness of training supervisors in human relations and (b) the relation of supervisors and employees. Researches became more complicated by studying more than one variable simultaneously. There was a trend to longer range longitudinal studies. Psychology is challenging the leadership of sociology in human relations research. (Some related studies by sociologists are included herein.) All this is good. Psychological research is tackling more important problems and doing it with some sophistication and with interesting results.

Human relations studies in industry are of much wider interest than to industry alone. They include follow-ups on attitude change and studies of relations among persons of equal and different status, which are important to human relationships wherever they may occur.

One trend of the year is the emergence of research organizations, especially at the Universities of Michigan and Ohio State, as dominating the research field in contrast to the efforts of individuals. The strength of such organizations is shown in their having broad integrated programs, the continuity of their longitudinal studies extending over several years. Their research productivity reflects their economic advantages of greater facilities for gathering and analyzing data.

On some topics it is difficult to evaluate the status of the art and science because of bias in the selection of materials that are published. Like the bridge columnists who publish only the hands that work, psychologists report, on some topics, mainly those instances where procedures have worked. For example, it continues to be extremely doubtful that publications in the area of performance ratings are representative. Even the fanciest psychometric systems frequently come upon evil days in their administration, but little if any of these negative instances find their way into the journals. On the other hand, it is probable, although less certain, that the opposite kind of selectivity occurs in the case of some topics. For example, case histories of supervisory human relations problems sound as if troublesome situations are presented more frequently than their representativeness may warrant. Very seldom is there a case reported of a smooth running supervisory relationship.

¹ The survey of the literature to which this review pertains was completed about May, 1952.

² Mrs. Rita Bakan assisted in compiling the bibliography and in abstracting some references.

It would not be so interesting. This, incidentally, appears to suggest an old familiar warning as to one of the limitations of the case history method.

By far the bulk of the research and publications in industrial psychology continues to come from universities. There are notable exceptions of persons in industry publishing, but they remain exceptions. The relatively large number of persons employed as consultants to industry has contributed almost nothing during the year to the psychological literature. Their work is apparently not research, or they are too busy making money to stop to write up their reports; probably the former is the reason.

The volume of literature continues to be large and perhaps is increasing. By national groups, the United States continues to publish the most. The references at the end of the chapter, however, do not give a fair national comparison. United States' literature was more carefully covered than that of other countries. England and Australia continue to fill one journal each mainly with industrial psychology. France is resuming a fairly brisk publication. The Australian writers concentrate on surveys and case studies which might be expected to shift to more experimental research as they complete their surveys of the situation. There is a suggestion in the English and Australian topics reported of a political influence on their choice. More preoccupation with joint consultation, for example, may be a result of Socialist governments.

American choices of research topics may also be influenced by the political atmosphere. The growing concern over studies of democratic processes in work groups and the degree of democracy of the leader are probably traceable to the political climate as perceived by psychologists. Fortunately the research publications on these topics are no more guilty of observable bias in the interpretation of results than are less politically charged topics.

Some military research deserves a place in this chapter for two reasons. A good deal of it has so obvious immediate application to industrial problems as to make it of interest to the industrialist. Since there is no chapter on military research, the *Annual Review of Psychology* has to recognize the significant developments on military problems by dividing the literature among the various chapters on the basis of relatedness of topics. Military organizations now have much more psychological research under way than do industrial organizations. The military program including contractors probably even now exceeds university manpower on nonmilitary psychological research.

The flow of military psychological research from journals has not yet reached a crest. A considerable volume of military research has found its way to reports circulated primarily within the military establishment. These are not reviewed in this chapter. A much smaller amount of military research has reached the psychological journals. This has been reviewed when the articles fit the outline for the chapter. Prominent among such articles are several studies on human engineering, mainly dealing with equipment design. Within a year or two the number of military publications in the psy-

chological journals will be much greater as the publication lag is overcome and as the military research organizations turn out their reports at an even faster rate.

Aside from the emphases mentioned in human relations and in military problems of equipment design, there were no other major developments during the year. Selection studies continued to bulk large with some effort being made to refine methods for jobs previously studied plus a few attempts to develop selection devices for jobs in which there had previously been no attempts to develop scientific selection.

Chambers' book (28) is a compact, fundamentally complete and easily readable introductory text which explicitly emphasizes the British literature. Vocational guidance and training methods are stressed in preference to the more usual accent on selection tests. The modern social point of view is prominent.

Laird & Laird's *Practical Business Psychology* (79) is similar to other books by the senior author in that it is popularly written. There are attention getting figures. It has a number of human interest stories, some from business and some from history. The authors show an acquaintance with the latest literature as well as a selection of the most important literature. The book is somewhat broader than its title describes in being about psychology in general applied to human efficiency and group living.

Ghiselli's chapter "Psychology in Industry" (49) is his usual careful and scholarly portrayal of well selected material. Outstanding is his table of "Representative Validity Coefficients of Various Tests in the Prediction of Job Proficiency for Different Occupational Groups" taken from one of his studies, previously published but not widely circulated.

Also by Ghiselli (50) is a selection of six "New Ideas in Industrial Psychology": (a) the motivation of workers by group discussion as proposed by Lewin and his colleagues; (b) factors in worker morale as determined by Likert and Katz *et al.* at the University of Michigan; (c) studies of leadership by Shartle and associates; (d) role perceptions of management and labor towards each other as conceptualized by Haire; (e) Flanagan's critical requirements as a method of work evaluation; (f) Robert Thorndike's formulation of the classification problem in terms of maximizing the number of adequately placed persons rather than selecting separately for each job without considering the total population of supply and demand. The selection is interesting and wise in terms of the most important current and recent studies.

Flanagan (44) has described psychological principles and procedures applied to military problems. He advocates an over-all program to take into account all objectives to which psychology can contribute. There is a presentation of principles and procedures applied to (a) selection and classification, (b) training, (c) measuring proficiency, and (d) evaluating job performance. He advocates the critical incident procedure for establishing classification procedures for new jobs. He does not seem to recognize that the criti-

cal incidents method, like all job analysis, does require validation of selection procedures.

HISTORY

Thurstone (123) edited a volume of essays to honor Walter V. Bingham on the occasion of the latter's reaching his 70th birthday. A central theme is to point out the influence of the first department of applied psychology which was headed for a time by Bingham. Bingham's place is outlined as a pioneer and salesman for psychology applied to industry and to the military service. This book will be of interest to his hundreds of friends throughout the country.

Ferguson (41) has traced in detail the influence of Bingham and the group at the Carnegie Institute of Technology on the applications of psychology to the insurance business, which incidentally, probably constitutes the most impressive single application of industrial psychology. The case is convincingly put that much of present day personnel research has a direct and close link to the innovations of Bingham and his able colleagues and students of the 1920's.

OCCUPATIONAL INFORMATION

Occupational trends in the United States from 1940 to 1950 were found by Kaplan (73) to show increased proportions of white collar and manual workers compared to the total working population and decreased proportions of service and farm workers.

Three studies showed that the number of variables employed in job evaluation systems could be reduced and still account for almost all of the variance. These were by Grant (53), Oliver & Winn (98), and Rothe (109). The results confirm what previous analyses had found.

Wagner (131) used the critical incidents method in analyzing three air crew jobs. The incidents were classified into 24 elements. Different patterns resulted for pilots, flight engineers, and radar observers. Two teams operating independently produced results that agreed. The results are to be used in assigning selection test weights. This method is said to be superior to ordinary job analysis in that it uses a smaller volume of data and in that relatively inexperienced observers can gather the data. The estimated selection test weights will still require validation.

INDIVIDUAL DIFFERENCES

The theory and general problems of selection research are ably reviewed by Bechtoldt (11). His chapter is in the *Handbook of Experimental Psychology* where ordinarily one would not expect to find such nonexperimental material. Bechtoldt's chapter and thorough bibliography constitute good material for graduate students who are preparing for personnel research in industry or elsewhere.

Cleeton (30) proposed the use of the clinical method in employee selec-

tion in contrast to a rigid psychometric approach. The present writer would grant that on occasions a clinical approach has to be used on account of small numbers of cases or because of the undeveloped stage of selection procedures. He would also grant further, for the sake of discussion, that some variables can still be measured better by clinical procedures. However, the usual statistical methods must still be used to validate the clinical procedures. To such validation Cleeton seems to object.

Halsey (58) has written a book, *Selecting and Inducting Employees*, intended primarily as a handbook for executives. The text presents an introductory account of current procedures with emphasis on tests. A long appendix, 127 pages, presents many personnel forms and sample test items. The book will be of interest as a reference in a first course on industrial tests.

The correlation found by Gaylord *et al.* (48) between production records of clerical work and ratings was only .48 to .55. The authors conclude, and justifiably, that this relationship is too low to trust ratings by themselves to give a sufficiently accurate index for production.

Satisfactory criterion measures were developed for proofreaders by Lawshe & McGinley (84) and for chocolate dippers by Rothe (108). Speed and errors for proofreaders each could be measured reliably but were independent. Production of chocolate dippers over a 16-week period correlated highly from week to week.

Having a personnel technician sit with a rater was believed by Taylor & Manson (122) to produce more valid ratings although they had no control group to prove it. Reliability coefficients for statistical clerks, schedule editors, key punch operators, and stencil typists varied from .57 to .82, which under the circumstances were judged to be satisfactory. Additional confirmation of the benefit of the procedure was said to be found in relatively low correlations between traits.

Selection by interview and application blank.—A survey by Fields (42) showed that 44 agencies were using or had used the group interview as a selection device for supervisory and administrative positions. Correlations of ratings from the group interview with ratings from individual interviews were so low as to make it appear that different things were being measured. No results are given to show what the group interview does measure or whether it actually predicts supervisory success.

Crissy & Regan (32) reported a method for studying halo within the interview. Four executives interviewed 82 applicants for executive training and wrote independent reports on each interview. The authors analyzed these reports in terms of evidence for judgments expressed. In support of identical judgments concerning separate traits for an accepted and a rejected group, there were significantly more units of negative evidence for the rejected group. For example, for the judgment of "average" in drive, second from low on the four point scale used, candidates who were rejected received more negative evidence in the interviewers' notes. The article appears to be a worthwhile contribution of thoughtful hypotheses applied to a commendable amount of data.

Personal data items of department store saleswomen were studied in relation to turnover by Mosel & Wade (96), and in relation to cost of sales by Mosel (95). In one department store, such items discriminated between saleswomen who stayed on the job for at least a year and those who stayed on the job no more than six months. To the extent that information was available, the valid items from the first store were tried in a sales population of a second department store. The three most valid items in the first store were also valid in the second store. These were age from 35 to 54, education, and domicile. Age was the only item which was valid for the best saleswomen in terms of turnover and also sales costs. Saleswomen stayed on the job longer who had completed not more than 12 years of school, but they had lower sales costs if they had had between 13 and 16 years of school. Keeping house was the valid domicile for long service saleswomen; the low sales cost ones characteristically lived in a boarding house.

Levine & Zachert (86) reported correlations of scores on eight scales from a biographical inventory with training grades in each of 24 Air Force technical schools. A number of correlations were highly significant, and because of their low intercorrelations with other predictors in the Airman Classification Battery, the biographical inventory contributed significantly to the prediction of school success.

Personnel tests.—Green (55) compared interest and personality test scores of Patrolmen in a Bureau with those of men who wanted to transfer to the Bureau. Ability test results and other evidence pointed to the two groups being comparable. The applicant group appeared to fake their responses on the Kuder Preference Record and on the Guilford STDCR personality test. Strong (121) found the vocational interests of college seniors to correlate .75 with their interests 22 years later. Employees in eight organizations were favorable to employment tests as reported by Van Zelst, Kroh & Darr (129).

Ghiselli & Brown (51) tabulated validity coefficients for tests in predicting training records as reported in the United States since 1919. The mean validity for the most predictive tests of each occupation was .42. For the most part, tests were not differentially valid, e. g., spatial relations tests were almost as valid for clerical as for mechanical jobs.

Oxlade (99) obtained a coefficient of correlation of .77 between the Minnesota Paper Form Board and a criterion of progress on the job for power-sewing machine operators. Laney (80) obtained a multiple R of .77 between a test battery and supervisory ratings of gas service appliance men in Brooklyn. Tests which contributed most in order of validity were Wonderlic Personnel, Bennett Mechanical Comprehension, and Bennett Hand-Tool Dexterity. With a similar occupational group in Washington, the three tests which contributed most in order of validity were the same as for the Brooklyn group.

Case (25) correlated test scores with training school grades for 28 employees in an aircraft design class. Coefficients of correlation were .56 with

the Survey of Space Relations ability and .55 with the Otis Self-Administering Test of Mental Ability. Neither a Bennett Test of Productive Thinking nor the Miller Analogies correlated significantly with supervisory ratings of engineering supervisors as reported by Webster, Winn & Oliver (133).

One hundred thirty-one toolmakers engaged in the five jobs of fitters, turners, millers, grinders, and inspectors were given a battery of 20 tests by Bonnardel (17). The tests were manual ones aimed at measuring mechanical aptitude. Profiles of scores were drawn in each job group for persons rated "good" and "less good." Only for turners did the profiles show any apparent differences between good and poor employees.

Electrical substation operators were studied by Goguelin (52). The amazingly high coefficient of .90 was reported between supervisory and staff ratings for 45 operators and results from a battery of 16 test scores. The tests were verbal and technical intelligence, attention, and memory. Analysis of variance results are given for each test and the criterion rating. Perhaps the combination took advantage of chance errors so that there would be considerable shrinkage with a new sample.

Clerical aptitude tests were factor analyzed by Bair (6) who identified factors of (a) perceptual analysis, (b) speed, and (c) comprehension of verbal relationships. The Minnesota Clerical Test showed higher saturations in these factors than other tests analyzed. Clerical aptitude tests were valid and the Wonderlic Personnel Test was invalid for predicting success of key punch operators [Hay (61)]. Blakemore (16) found that typing performance was correlated .62 with the Hay Number Perception Test and also with the Minnesota Number Checking. Kriedt (78) developed a test for correspondence clerks consisting of items which involve selecting the most and least preferred of three paragraphs. The test showed significant validity when it was administered to a second population.

Work samples of driving skill for light, medium, and heavy vehicles were correlated with the results of paper and pencil and performance tests by Bartelme *et al.* (10). None of the correlations were very high. Steering performance was the most consistent criterion for the three different vehicle weights, although its relationship to work samples was only slight.

Holmes (62) reported a study of guards in an arsenal. Scores from two subtests of the Army General Mechanical Aptitude test, (a) Technical Reading and (b) Paper Form Board, had an *R* of .67 with supervisors' ratings.

Tests for supervisors.—A follow-up study on the prediction of foremen's success was made after an interval of almost two years by Castle & Garforth (26). Supervisory ratings for 44 foremen correlated .68 with the selection procedure including interview assessments and test scores.

Arbous & Mardee (3) obtained a correlation coefficient of .60 between selection procedures and supervisors' ratings of the success of administrators. Included in the battery predicting ratings were two group discussion methods: (a) assigned leadership and (b) leaderless group.

Jones & Smith (70) correlated responses to items in a test with ratings of foremen made by supervisors in one company and by subordinates in a second company. The test was composed of items relevant to (a) supervisory problems, (b) personality, and (c) personal history. Significant correlations were obtained in each instance. Tests correlated higher with employee than with superiors' ratings. With both sets of raters the order of correlation coefficients by item types was the same from high to low as the order in which they are listed above. No cross validation was reported.

Knauff (77) found insignificant correlations between a cost criterion for bakery shop managers and existing Strong Vocational Interest Blank scales. From the Strong items Knauff constructed a new scale for bakery shop managers on which scores correlated .53 with a criterion of success for an independent group of 32 managers and trainees.

Sparks (117) showed insignificant correlations between supervisors' ratings of oil refinery foremen and scales from the Bernreuter Personality Inventory. The study is noteworthy in size of population for foremen, 360, of which results were analyzed for 191, and in the reliability coefficient of the ratings, .91.

Tests for salesmen.—Insurance agents' income was shown by Ferguson (40) to be related to the effectiveness of their management, carefully determined, as well as to aptitude. Aptitude predicted incomes best where agents were supervised by good management. The Strong Vocational Interest Blank scale for Life Insurance salesmen was demonstrated by Bills (15) to predict production in selling life insurance regardless of age, education, and previous experience in the same type of work. Strong Vocational Interest Blank profiles differentiated successful from less successful salesmen and salesmen from research workers in an aluminum company as reported by Webster, Winn & Oliver (133). A sales situation test was constructed by Rock (105) who showed that salesmen scored higher on it than did production supervisors. Salesmen selling to consumers scored higher than salesmen of technical products.

HUMAN ENGINEERING

Equipment design.—There is a thorough chapter on human factors applied to equipment design by Fitts (43). The literature reviewed seems reasonably complete in covering not only material since and during World War II but also related experiments which date back almost to the beginning of psychology in the United States. Where actual equipment is used for experiment or other study, it is almost exclusively military and for the most part involves airplanes. This represents the research emphasis at present, although there have been more industrial studies conducted than are included. In spite of the emphasis on military applications, the methods and generalizations will frequently be directly applicable to industrial problems, although the industrial questions of equipment design are less critical than those of military aviation. The latter give more emphasis to the importance of human errors under conditions of sudden stress and of low illumination.

Roscoe (106) showed that pilots could fly as well when looking out through a periscope as when flying contact, provided the periscopic image was as large as an 8-in. square. Performance deteriorated as the image became smaller. Pilot performance was improved with increases in the angle of the outside world shown. There are two possible implications of this experiment: (a) It shows the feasibility of flying in a situation intermediate between contact and instruments which might be used directly in high speed aircraft where windshields are impractical. (b) It gives leads as to the background for a desirable display of the horizon in instrument flying.

Ellis (36) found that the optimum height of the work surface was 42 in. in a block turning task. Barber & Garner (9) showed that scales were read more accurately when numbered in 10's or 20's rather than in fives. There were tendencies to read by whole numbers and to respond to the mid-point between markers.

The theory and methods of analyzing errors in man-machine systems have been described by Chapanis (29). He uses radar as his main example, but the methods can also be applied to industrial systems. Equations are derived for accumulating constant errors, accumulating variable errors, and accumulating constant and variable errors. Such methods can be used to determine whether it will be fruitful to attempt to reduce errors by the redesign of equipment and whether to change adjustments in machines.

An instrument for measuring time in conducting motion studies was invented by Davis, Wehrkamp & Smith (33) which records separately time for manipulation and for response. Up and down movements were found to be faster than those from side to side. Right-handed subjects were faster than left-handed. Among right-handed subjects the left hand was equally fast in travel but slower in manipulation.

Kappauf & Smith (74) studied the relation of the number of units on dials to errors and to speed of reading the dial setting. The smaller the size of the scale unit, the more errors in reading. Speed of reading was retarded by an increased number of dial units.

Jenkins, Maas & Olson (69) studied the effect of inertia and other variables on time required to set a scale with a knob. In matching settings on a scale by turning a knob, inertia was of little importance. When backlash was added to inertia, it took slightly longer to make adjustments. Inertia overcame some of the disadvantage that friction made in making adjustments.

Training.—Wolfe (139) has written a chapter on training which gives a good review of laboratory and military studies. There are a few references to industrial studies. The paucity here is a fair indication of the lack of industrial experiments or other studies giving concrete data.

An evaluation of the training of supervisors in human relations is referred to by Mann (89) who mentions mainly in this context the doctoral thesis of Hariton. Hariton (60) describes his evaluation of the effectiveness of training foremen in human relations by measuring the attitudes of employees toward supervision before and after training. In two divisions where foremen had been trained, employee attitudes improved in one and became

worse in the second. Hariton ascribes the difference to the different situations, or specifically to the differing attitudes on the part of the foremen's supervisors in the two divisions.

A checklist for the evaluation of human relations training was developed by Di Vesta *et al.* (34) for officers attending the Air University. The checklist used by class members upon one another was internally consistent and also correlated significantly with instructors' ratings. The items, which are not given in the article but which may be obtained from the American Documentation Institute, appear to be as applicable to industrial as to military training.

Lawshe & Cary (82) found that training time in assembling mechanical parts was not improved when college students repeated their instructions once orally. Evans (38) trained inexperienced employees to read a micrometer so effectively in 7 hr. that their errors were much less than those of experienced toolmakers. Errors made by experienced toolmakers had frequently exceeded the expected tolerances.

Bellows & Rush (12) trained business executives to read faster and comprehend more accurately. Although the average changes were statistically significant, practically they were slight.

Illumination.—Tinker (125) showed empirically that the minimum illumination necessary for maximum speed in reading was seven foot-candles for newspaper type and 3.1 foot-candles for book type. These values were greatly under the computed minimal lighting advocated by Weston & Crouch.

Accident prevention.—Keenan, Kerr & Sherman (75) correlated five-year accident records in 44 departments of a tractor factory with ratings on 10 variables. These variables were ratings of conditions which were hypothesized to be related significantly to accidents. Coefficients of correlation significant at the 5 per cent level were found for 4 of the 10 variables. These were in order of their significance from high to low, (a) uncomfortable shop environment; (b) manual effort involved; (c) greater degree of operational congestion; (d) greater degree of obvious danger, and (e) lower promotion probability. Accidents, it was said, were not usually caused by obvious danger but such danger led to a concentration of attention such as to bring about disregard for less obvious hazards.

Kerr had confirmatory evidence from another company of the above relation of accidents to low promotion probability. The authors advance conclusions only tentatively, and well they might. While this approach is interesting, it requires a thorough study of the situation plus one or more cross validations to minimize the likelihood of spurious relations being mistaken for causes.

Accident records over a three-year span correlated $-.54$ with desirability values from sociometric choices among 90 steel mill employees according to Speroff & Kerr (118). Their favored explanation is that rejected workers worry and this leads to accidents. They admit a second possibility of a general factor which may account for accident proneness plus lack of skill in

dealing with people. Persons with variable reaction times were found to have no more automobile accidents in a study by Cation, Mount & Brenner (27), than those whose reactions were less variable.

HUMAN RELATIONS

Sartain (113) points out that psychologists have had little to do with the present interest in human relations, but have continued to concentrate their research on less important but more accessible research topics. He advocates a shift in emphasis. It is true in general that in the past psychologists have contributed little to human relations study. The investigation of this thriving topic has been led by sociologists, educators, businessmen, and others outside the profession of psychology. This year, however, psychologists have done a significant amount of work, particularly in the evaluation of the effectiveness of human relations training.

One major study in human relations headed by a psychologist was done in England and has been reported in a book by Jaques (68), *The Changing Culture of a Factory*. A team of social scientists studied the personal relations of factory employees and management over a period of several years. There was access to all levels of personnel from the general manager to first line employees. Management from the beginning was highly progressive and wanted to become even more democratic. Some of the difficulties in this process are shown. Top management, in striving to establish successful communication with first line employees, alienated middle management for a time. The head man in the company tried to give responsibility to his immediate subordinates who in turn were somewhat baffled and frustrated by this relatively sudden democratization. The author gives a clinical as well as social-psychological interpretation.

When management and employees were led to an understanding of their situation by Jaques, they developed a different democratic representative organization which apparently was going to bring about even more effective performance. All along the company had been financially successful. Although the factory was in England, the book is directly applicable to much of industry in the United States. It will probably apply best to those industries which have already been trying to introduce human relations or multiple management. It will show some of the pitfalls of further development and also give by implication suggestions of successful lines of change.

A second edition of the book edited by Hoslett (63), *Human Factors in Management*, is a major contribution which reprints a number of articles written since the first edition. A sociologist, Dubin (35), has edited an important book, *Human Relations in Administration*. This book is similar to Glover & Hower, *The Administrator* (51a), in presenting a number of actual case histories. The histories are interesting and hard to obtain. Unlike Glover and Hower, Dubin presents a complete theoretical framework for his subject.

Canter (22) endorsed the use, in studying the effects of human relations training, of an experimental design in which a second control group is used.

By so doing it is possible to determine the effects of pretests prior to training. He gives some data on training of insurance office supervisors which have been analyzed using two control groups.

The use of the member-centered conference method is advocated by Argyris & Taylor (4) to study human relations. They quote some records of a conference of industrial supervisors to which they point as being productive. Role is advanced as a useful concept in the study of complex organizations by Jacobson, Charters & Lieberman (65). Five role concepts were discussed.

The problems of a research program in relation to an organization in which it is conducting research were discussed by Jacobson *et al.* (66). The following principles were adopted: (a) research findings must be available for publication; (b) research will not be used to solve immediate organization problems unless in turn it is a part of more generalizable research; (c) no research will be started unless all related groups want it.

Incentives.—A review of the trend in profit-sharing in Australian industry was reported by Hurley & Wickham (64). This study was made in 1951 and compared the situation with 1947. There was a slight increase in incidence of profit-sharing plans. It was not possible to determine the effectiveness of profit-sharing, but it was judged to be less than expected by management.

Lincoln's *Incentive Management* (88) is a book whose appendix will be of more interest to industrial psychologists than its body. Included are data comparing employee earnings and employee costs between the Lincoln Electric Company and competing companies. In spite of paying their employees approximately twice the amount paid by competing companies, the Lincoln Co. had lower employment costs. This may be due to the absence of work stoppages and to much lower turnover, for which records are also given.

There is disagreement as to whether the net long range effect of wage incentives is positive or negative. Experience of eight companies with wage incentive plans in Australia was described by Kangan (72). Production increased in all companies, the highest increase being approximately 100 per cent. The increase was greater in the six plants where the plan called for payment on individual output than in the two plants where payment was on an over-all factory basis. Five cases were presented by Roy (111) as evidence that higher costs to management eventually result from wage incentives. Wage incentives cause employees to try to beat the plan and to concentrate on various stratagems for getting more money without producing more. The overhead involved in operating such systems is a cost not to be overlooked.

Stone (120) described the reactions to wage incentive systems of employees in two companies. In one, a store, there was no restriction of output, whereas there was in the second, a factory. The author advances the explanation of greater mobility as the reason why restriction does not occur in the store. He admits the possible importance of two other explanations, social distance and job scarcity. Campbell (21) found that as the size of the working group increased where wage incentive plans were present, knowledge

of results and output decreased, and among those workers who had no knowledge of results, dissatisfaction increased.

Roy (110), as a participant observer, found in a machine shop that all operators restricted their output by wasting 2 to 3½ hr. per 8-hr. day. Restriction was somewhat different under three circumstances, (a) quota restrictions under piece-rate pay plan where restrictions were calculated to prevent rate cutting; (b) not trying to reach a quota under piece-rate plan where highest effort would have barely paid more than day rate; (c) slowing down on a job where piece rates had not been set.

A department store installed an individual wage incentive payment plan for salesmen, which according to Babchuk & Goode (5) was accompanied by high production but produced low morale via the hostile acts of competing salesmen. Salesmen gradually worked out a procedure for pooling their efforts so that commissions were identical. Sales stayed up and hostile acts were eliminated so that morale became high.

Conferences.—Peterman (102) studied satisfaction with conference decisions through observation, interview, and questionnaire. Actual conferences and conferees in industry and in government were studied with the following conclusions, some of which are more descriptive than explanatory. Satisfaction by participants with the results of conferences is decreased by spending more time per topic or per meeting. Satisfaction with conferences is increased where (a) a greater proportion of items are completed, (b) the goals of the conferees are similar, (c) the conference group is perceived by the conferees as unified, (d) the participants support one another, (e) the conference is centered in the leader rather than in the group. It is only a slight exaggeration of the above to conclude that a good conference is one where all of the work has been done in advance of the meeting.

A comparison of the effectiveness of use of group decision and of lecture in improving ratings of supervisors was made by Levine & Butler (87). Prior to the study it was found that a tendency existed in a manufacturing plant for supervisors to rate experienced employees in a high labor grade too high and inexperienced employees too low. Supervisors were divided into (a) a control group; (b) a group using group decision to attempt to change rating behavior; (c) a lecture group to attempt to change rating behavior. No significant change occurred in (a) and (c). Ratings by the supervisors using group decision changed significantly in that the members of the high labor grade were rated lower than before. Although the number of cases is small, this is a demonstration of the possible effectiveness of group decision to a very important industrial personnel problem.

Attitudes and job satisfaction.—That historians have planted attitudes unfair to businessmen is the thesis of Saveth (114). He holds that historians have in general been "feminine idealists." This he believes explains why they have overemphasized the wrongdoing of American businessmen.

The "Use of Attitude Surveys in Personnel Practice" is reviewed by Moore (93) in a chapter which includes a fairly well selected bibliography of 35 items containing several recent references. The treatment is too brief to

be thorough. The use of attitude survey results in a public utility is described by Mann (89). There are no quantitative studies of change but several impressions emerged from experience and are stated as hypotheses. These are that changes are more likely to occur under the following conditions: (a) Superiors are concerned. (b) There is a high degree of participation. (c) There is self-analysis. (d) There are objective data about one's own organization. (e) Timing in the use of results is dictated by the users. (f) Group discussions occur on the use of results.

Employee satisfaction was rated reliably from interview records, but was rated no more reliably with increased interview experience and training in psychology [Carey, Berg & Van Dusen (23)]. A job satisfaction scale developed by Brayfield & Rothe (19) had a reliability coefficient of .87. It differentiated between two groups in a personnel psychology course, one of which was employed in personnel work (higher on the scale), and the other group composed of persons who were not employed in personnel work. Questionnaire results from an attitude study were compared with interview results from the same employees by Wedell & Smith (134). Interviewers rated the attitudes towards two of three topics significantly higher (5 per cent level) than the questionnaire results had indicated. The authors interpret their evidence as arguing for objective measurements of attitudes and away from the use of interview.

Results were reported for the attitudes of men in a number of English motor car and rolling mill factories by Walker & Marriott (132). Of the men interviewed, 59 to 75 per cent expressed themselves as being "fairly satisfied" or "satisfied" with their job. There was more boredom expressed by men working on assembly lines than others. Employees in two department stores were given Jurgensen's Job Preference Blank by Hardin, Reif & Heneman (59). There was a slightly different order of ranking of factors by men and women. Test-retest reliability was .98. Importance of items to job satisfaction of 40 retail store employees were studied by Haire & Gottsdanker (57) using the methods of interview and story completion. There was general but not perfect agreement between results obtained from the two methods.

Van Zelst (127) had construction workers fill out Kerr's Tear Ballot for Industry and rate each other on desirability as a work partner. Interpersonal desirability and job satisfaction had highest loading in the most prominent factor. This was labeled a general "job adjustment factor." A second factor was called "favorable attitude toward company."

Morale and monotony.—The relationship of morale to productivity is discussed by Kahn & Morse (71). It is pointed out that morale and productivity are not always perfectly or even positively correlated. Morale has been found to involve five components: satisfaction with job, immediate supervisor, and reward system of the company, plus involvement in immediate work group, and identification with the company. The relation of size of task to job satisfaction and efficiency was studied by Cox & Sharp (31). They concluded that employees on time rates preferred to be given an amount of work at a time such that they could finish in 1 to 1½ hr.

Turnover among employees in a textile plant was studied by Marrow & David (90) in relation to production. Highest turnover was found among learners who had almost, but not quite, reached the required production standard. It was thought that frustration was causing employees to quit. A training program was therefore developed which de-emphasized the eventual standard and contained easy subgoals. After this program was in effect, average monthly turnover decreased from 14 per cent in 1944 to 5 per cent in 1947.

Telephone operators and service representatives who had resigned were compared by Wickert (138) with employees still on the job. They reported more frequently that they had not had much chance to make decisions and had not felt that they had made much contribution to the success of the company.

The cost of labor turnover in Australia has been carefully calculated by Allen (1) and Felgenhaur (39). Among two groups of low-skilled factory groups, the estimated cost of turnover per person in one firm was approximately 96 dollars and in another was approximately 74 dollars.

Byrt & Wall (20) reported the first of a series of case studies in which they gave a method for and results of estimating the cost of absence from work. During a six-month period they estimated that absences of 3 hr. or more in a light engineering factory employing slightly less than 1,000 employees cost the company approximately 40 thousand dollars in net profits.

Mental hygiene.—Adjustment of West Indian Negroes who moved to England where they were discriminated against was positively correlated with their level of skill by Richmond (104). He points out that other variables, i.e., age, intelligence, and status might have been important.

The employability of high-grade feeble-minded, i.e., mean IQ of 72 on Terman-Merrill Vocabulary, correlated .67 with a combination of an Instability Rating and the United States Employment Service Manual Dexterity Test in a study by O'Connor & Tizard (97). Job adjustment problems among 80 cases of mental defectives were found by Peckham (100) to include in order of frequency: lack of acceptance by fellow employees, i.e., teasing; lack of social and vocational sophistication; salary dissatisfaction; inability to budget properly; lack of initiative and job responsibility.

Hearing loss correlated $-.42$ with job satisfaction where age was held constant in a report by Zintz & Kerr (140). The result should not be considered final until and unless it is confirmed on an independent sample. Older part-time workers in a department store were found by Stanton (119) to show less turnover and to receive more wage increases based upon merit. No doubt older workers are not so likely to find full-time employment.

The effect on turnover of counseling employees was reported by Weider (135). Forty-one employees whose counseling was considered effective remained on the job an average of 10.8 months. Seventeen additional employees whose counseling was judged to be unsuccessful remained an average of 3.3 months. Judgments as to the success of counseling were made partially on the basis of job adjustment so they are not entirely independent of the

turnover measure. A control group of 29 employees with emotional problems who were not counseled remained on the job only 2.5 months. Unfortunately, it is not clear that the control group is equated to the counseled group in severity of emotional troubles.

Worries of union leaders at various ages were reported from memory to Van Zelst & Kerr (128). The age of greatest worry was just over 30 years. By far the most common worry was economic.

Labor relations.—The most interesting study of labor relations to appear during the year was conducted by Rose (107), a sociologist. He with his students carried out a large number of structured interviews with a representative sample of members of an American Federation of Labor union in St. Louis of Warehouse and Distribution Workers. The union members studied worked for 15 wholesale companies. The main interest of the study was on the internal cohesion of the union.

A human emphasis on the relations between union representatives and union members is made by Garfield & Whyte (47). The senior author is Vice-President, International Chemical Workers Union, American Federation of Labor. The desirability of giving a complete human story to the local of what occurred in the bargaining session is expressed. It is asserted that unselfish motives, e.g., the effect on other unions, can under some situations be more effective than the selfish interest of more pay.

Communications.—Several studies were reported on the readability of handbooks and other publications and of contracts. Two follow-ups, Knauff (76), and England (37), were reported on attempts to improve publications. A weekly employee publication was changed in an attempt to vary its content to include more informative, descriptive, and historical material, and also to increase its reading ease and human interest (76). Readership studies were made before and after the changes. After the changes there were significantly more respondents who reported that they took the publication home to their family, and regarded the publication as excellent. The percentage of employees answering that they read the publication did not change. A comparison of reading ease before and after changes in publications was shown by England (37).

Studies of employee handbooks were made by Lawshe, Holmes & Turmail (83), and by Carlucci & Crissy (24). A study of 84 handbooks consisted of a content analysis, ratings of visual appeal, reading ease, and human interest. Ratings of visual appeal placed the best handbook as one which followed the format of *Life* on a reduced scale. A comparison can be made on the Flesch scales between the results of Lawshe, Holmes & Turmail and Carlucci & Crissy. The former was a sample of American industry in general, the latter of the "Billion Dollar Club," i.e., companies with either gross assets or annual sales exceeding one billion dollars. The Billion Dollar Club's handbooks were somewhat more interesting.

Union contracts were found by Tiffin & Walsh (124), and by Lauer & Paterson (81) to be too difficult for easy understanding. A new approach to communication study is reported by Jacobson & Seashore (67). They give a

hypothetical structure and a method for studying communication practices. It is thought that communication practices will reveal important characteristics of the organization.

Joint consultation.—Two accounts of joint consultation give favorable reports. A report by Veness (130) concludes that joint consultation was effective in solving problems. Another similar description of joint consultation is Graves' account of experience in two Australian factories (54). This is the second of a series of Australian case studies on the topic. Joint consultation worked better than it often does in these two factories. Graves thought that the explanation was that management had in the beginning delegated to committees the incentive pay plan and the suggestion system which were of continuing interest to everyone.

Two eulogies of outstandingly successful labor relations are given by Schmid (115) and by Mockridge & Prall (92). These two cases are similar to a number which have appeared in previous years which as human interest stories attract attention and interest in the possibilities of the human aspect in labor relations.

Less common than pointing with pride is viewing with alarm in industrial psychology, but there is one example of this in Bennett's book (13) which describes unfavorable labor relations at one time in the Ford Motor Company. It shows the suspicion and ill-feeling that occurred following a labor policy of spying and force.

Supervision.—A broad book on supervision has been written by Piffner (103). There is an extensive and up-to-date bibliography from psychology, sociology, anthropology, management, and personnel journals. The topics covered are so broad as to cover the major part of industrial psychology, a coverage justified by the supervisors' wide responsibilities. Psychologists may question whether it is wise to lead the supervisor to believe that he is a clinician and a placement specialist. Since the industrial psychologist performing clinical work or placements duties is the representative of some supervisor, the issue is thus not clear-cut but is rather one of balance.

The Harvard Graduate School of Business Administration has published another book of case studies. *Executive Action* by Learned, Ulrich & Booz (85) is an interesting little book based on interviews with top management and employees in 12 companies. The investigators' original intent was to study line-staff relationships. Their scope was changed when they found management wanted to talk about a wider range of executive problems. *Executive Action* has a systematic interpretation of executive behavior, an interpretation which is conservative and wise. In Harvard's enthusiasm for the case method as a teaching device, there appears to be some danger of the method's retarding research by slowing down the use of more exact scientific methods.

The behavior of leaders was found in a factor analysis by Shartle (116) to be composed of three factors, behavior that (a) increases a leader's acceptability to the group, (b) is high in production and organization, and (c) is effective in the interaction of group members.

The importance of leadership climate has often been suggested but only

recently have pointed efforts been made to measure its effects on first line supervisors. Fleishman (46) studied directly the attitudes before training of personnel at three levels. These were first line foremen, employees supervised by the foremen studied, and supervisors of the foremen studied. Foremen emphasized either human relations or production, depending on the emphasis of their bosses. The obvious implication is that for foremen training to be effective, it must at least be preceded by training for higher supervisors.

Fleishman also studied the effects of foremen training in human relations both immediately after training and later on the job. Immediately afterwards there was an increase in favorable attitudes to human relations and a decrease in emphasizing immediate production. Later on the job, the results were reversed, i.e., attitudes were less favorable to human relations, and behavior and attitudes tended to strive for immediate production gains. In other words, training produced effects opposite to its objectives.

Meyer (91) developed a careful criterion for foremen involving rating by supervisors. This criterion he correlated with each of four measures he had constructed plus the Wonderlic Personnel Test and Thurstone's test of Word Fluency. There were some differences in results among the various departments of foremen studied. Highest correlation with the criterion for the entire group of 190 foremen was .36 with a Social Judgment Test. This is a projective test in which the subject is called on to state how a supervisor, whose background is described, would respond under certain situations.

Pelz (101) analyzed the relations between employee attitudes and supervisory behavior in a large electric utility company. Attitudes were determined from a questionnaire distributed to employees, and from behavior as reported by first line supervisors in interviews. Supervisors were divided into two groups of high and low influence within the department, inferred from items of (a) reporting a voice in decisions made by superior, (b) not having to see superior frequently, (c) salary. Populations were also split on the basis of variables which had previously appeared to be significant, men versus women, blue versus white collar workers, size and union membership. In the groups with high influence foremen, correlations were in general significant between behavior attempting to help employees achieve their goals and employee attitudes. Among the low influence supervisory groups, correlations were zero or negative. Behavior within the work group will not bring about favorable employee attitudes unless the supervisor is influential with his boss.

Rupe (112) factor analyzed the items in a rating made by subordinates of supervisors on the Purdue Rating Scale for Administrators and Executives composed of 36 items in 10 logical groups. Two factors emerged. One which accounted for almost two-thirds of the variance measured had to do with behavior towards subordinates. The second factor was labeled "Executive Achievement."

Weschler, Kahane & Tannenbaum (136) administered a questionnaire to the members of two divisions in a Naval Research Laboratory. Supervisors were interviewed. The head of Division A was restrictive, the head of Divi-

sion B was permissive. The ratings by members of Division B were higher than those of Division A in job satisfaction, perceived productivity of the division and laboratory, and perceived morale. Perceived productivity of the work group was practically identical in Divisions A and B. Ratings by superiors and staff personnel agreed with those of division members with regard to their perception within the divisions of job satisfaction and morale. Superiors and staff personnel rated the productivity of Division A as better than B. The head of Division A was more frequently chosen as best researcher but less frequently as best administrator and best liked.

A study of decentralizing and centralizing supervisory control in experimental working groups within a large clerical organization was reported by Morse, Reimer & Tannenbaum (94). Three types of control were studied. These were making decisions, carrying out decisions, and punishment. People seemed to like having controls shifted down to them; among people of equal status it was difficult to give punishment.

According to a survey by Whyte (137) some corporations are now giving attention to wives not only by considering their qualifications when considering the husband to be employed or promoted as an executive, but also by training and indoctrinating the wives so that they will fit into the organization.

MARKET RESEARCH, ADVERTISING, AND SELLING

Market research.—A book on *Marketing Research* by Bradford (18) is a useful introduction but relatively uncritical for the research student. It gives little attention to the crucial problem of validity. The book's greatest strength is in giving a good bibliography of sources to industrial and governmental publications which furnish background statistics and other data about markets.

An attempt was made by Banks (8) to develop a Guttman type attitude scale for complaints of airline passengers. After the rejection of some items, eight dichotomous items remained with a reproducibility coefficient of .85. According to Guttman's arbitrary standard this means that attitudes in this area are not homogeneous. Passengers who had flown more had less favorable attitudes toward flying than those who had flown less.

"A Study of Interview Refusals" was made by Benson, Booman & Clark (14) which has implications for market research although the content of the interview was a public opinion topic. Refusals were not the results of the topic of the interview. On the first call to 230 homes, 136 interviews were completed. Thirty-three of the failures to complete the interview upon first call were due to refusals, the remainder to persons not being at home. Call backs were made until all persons were contacted, and attempts were made to complete interviews where persons had originally refused. Finally, interviews were completed in all except one of the 230 homes. The education level of those who initially refused was significantly lower than for the nonrefusals. This confirms the findings of a previous study.

Psychologists have given away a quantity of ice cream and beer during

the year. The coefficient of agreement can be used to show the degree of agreement and significance of differences in product choices when the method of paired comparisons is used [Balinsky, Blum & Dutka (7)]. Two French ice creams of different butter fat content were preferred over two domestic ice creams.

A consumer panel technique was demonstrated by Fleishman (45) to reveal some differences in brand preferences for beer. Forty-eight bottles of beer arranged in lots of eight bottles of each of six brands were left each day with each family of a carefully chosen sample. Different brands were in bottles with a characteristic color of bottle top which was changed from day to day. Daily records by brand were kept by families and by individuals. The panel ran for seven days. Each of two beers were consumed significantly less than each of the other four. Records on brand named from color of cap as liked best corresponded generally to the consumption records. Name preferences were more extreme than differences in consumption. The author does not point this out but it seems an important point to the reviewer. Under the circumstances it would appear that name preferences would be more sensitive than actual consumption. Frequently, a brand would be chosen by chance, whereas if it was not liked, chance would not enter in that manner in the naming response.

Fleishman's beer drinkers showed that some beers can be distinguished on the basis of taste and suggested that others can not. Of those persons who said that before the panel they preferred brands A and D, these brands were chosen during the panel in 56 per cent and 45 per cent of their choices respectively. On the other hand, similar results for persons designating preferences for brands B, C, and F were respectively 26 per cent, 29 per cent, and 13 per cent. These choices for the latter three beers could have occurred by chance.

Advertising.—Trends in advertising in *Time* and *Newsweek* were reported by Trenchard & Crissy (126). There were significant increases in the size, in the use of color, and illustrations. Each of these has been found to be an attention-getting variable. The reviewer suggests that their attention-getting power may wane because their original power might have been due in part to contrast.

The effects of reading fatigue and interview fatigue for magazine advertisements were determined by Hadley (56). Reading fatigue was varied by the thickness of a number of issues of each of two women's magazines. Interview fatigue was varied by having some respondents interviewed concerning only half the advertisements in a magazine, others concerning all the advertisements in the magazine. Results from women interviewees were analyzed separately for full page four color and for full page black and white. For the colored advertisements, interview fatigue was important and reading fatigue unimportant for differences in values of "Noted" and "Seen-Associated." The latter means that the interviewee states that she has seen the advertisement and associated it with the product advertised. On the other hand reader fatigue was important and interview fatigue unimportant for

colored advertisements. Black and white advertisements showed fatigue influences similar to, but not identical with, the patterns found among colored advertisements. These are interesting results concerning Starch values of magazine advertisements. The reviewer has found that Starch values, named for the psychologist originator, are highly regarded and eagerly sought by advertisers and advertising agencies.

Selling.—An unsigned article in *Fortune* (2) raises some interesting questions as to why people buy and points out that psychologists are coming closer to the answers. In spite of the United States' reputation for skill in salesmanship, it is pointed out that salesmanship alone cannot offset major economic depressions. The surveys of the University of Michigan's Survey Research Center for the Federal Reserve Board and the writings of Katona are referred to as the "most consistent work in 'psychological economic' fact gathering."

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COMPARATIVE PSYCHOLOGY¹

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This has been a good year for comparative psychology. First, there has been a long-needed revision of a standard text in the field (64). And second, two books have been published which summarize nicely the work of the European investigators in this area (32, 66). The latter books have the distinct advantage of being written in the English language and may therefore obtain a somewhat larger reading audience in this country than some of the previous publications by these two authors.

Of particular interest in the excellent third edition of *Comparative Psychology* are the chapters by Harlow (24), Smith (58), Hall (23), Collias (8), and Nissen (37). They represent, perhaps, the greatest change in the nature of the material covered in this text when compared with the two editions previously edited by Moss. Regrettable, however, is the fact that the recent developments in comparative psychology which have taken place in Europe seem to find no place in such a standard text. The name of Lorenz, the real founder of this new development, is not mentioned. Neither are the basic methods and findings of the constantly growing number of workers in this area of psychology.

It is probably high time that we stop deploring the lack of an orienting theoretical approach. We have one. A reasonably good first statement can be found in Tinbergen's recent book (66), while a brief summary is more readily available in this review series (39). Of course the title of Tinbergen's book may frighten away many "hard-headed" psychologists in this country, but it is really a straightforward statement of what Tinbergen calls "Ethology: the Objective Study of Behavior." The first chapter of the book covers the aims and method of ethology and discusses its relationship to other causal sciences. Other chapters deal with sensory processes; internal factors, both hormonal and neural; orientation responses and the "releaser—innate schema" problem originated by Lorenz. Chapter five is an attempt at a synthesis in which neurophysiological facts are integrated. Three additional chapters deal with development and learning, adaptiveness of behavior, and the evolution of behavior. All in all it is a good book, well illustrated, and containing a good bibliography of pertinent material.

Although Lorenz has not yet published his major opus, a rather charming account of his work and ideas has been published (32). It is really more than just another book on animal ways written by a naturalist because it incorporates much of his research findings and hypotheses. For those who may like an introduction to Lorenz in a painless dose, this may be a good place to begin.

¹ The material in this review covers approximately the period from May 1951 to May 1952.

SENSORY PROCESSES AND PERCEPTION

While most reports dealing with sensory processes and perception in animals are to be found elsewhere in the relevant chapters of this volume, brief mention of a few studies is made here.

Beecher (4), on the basis of anatomical evidence, proposes a substantiation of Ising's hypothesis. Ising argued that navigation of birds was made possible by the detection of Coriolis force. Beecher now suggests that the four vertical canals are inactive in flight and that a streaming of fluid in these semicircular canals may occur, owing to earth force, when the bird moves its head from side to side. This force could, theoretically, provide the migrating or homing bird with a compass. In addition, Beecher has found a membranous sac, capable of being inflated by venous blood and of covering the eardrum, which may be used as a means of cutting out unequal air pressure on the drums. This, he feels, may be done "when the bird is attempting to obtain the value for Coriolis force in a given locality."

In one of the latest of a long series of papers dealing with chemical reception in hens, Engelmann (16) discusses preference for green food plants. Added to the actual taste discrimination of hens in choosing preferred plants is the optical aspect of preference for plants with large leaves. Tactile cues which led to acceptance were smooth surfaces, easy tearing of the leaves, and a certain thickness. Cues which caused negative responses include hairiness, and narrowness of the leaves. Tactile and chemical cues were of major importance.

Gogel & Hess (22), working with chicks, found an innate preference for food objects having a less saturated color. By means of the usual method of testing color constancy (illuminating white food objects with an inclusive spot of red light to minimize constancy cues and using a general flood of red light for maximum constancy), they were able to demonstrate color constancy in chicks which had no visual experience prior to the test (choice) situation.

Working with sheep, Seitz (56) tested for pattern discrimination. A circle and a triangle, of equal area, were discriminated in approximately 50 trials, while twice that number were necessary for the discrimination between a circle and a square. Spontaneous preference for a larger figure was noted when the difference in area was at least 50 per cent. Seitz draws the conclusion that the sheep is by no means as alert or capable in the learning situation as has been reported for the related animal, the goat.

ORIENTATION

The old master, von Frisch (19), published a brief review of his work in which he added the more recent developments in his investigation of orientation and language in bees. While most of his report deals with the continued experiments with polarized light, he does report one item that is of more than passing interest to comparative psychology. Von Frisch had previously

noted that not all races of bees had exactly the same rate of the dance which signifies the distance from the hive to the food source. Yellow, or Italian, bees have a slower rate of dance when reporting a certain distance than the black strain has. This had earlier led to some confusion, since not all investigators used the strain which made up the material studied by von Frisch. The particular fact of interest, however, is that von Frisch observed the dancing behavior in a hive in which the black queen had been hybridized with the yellow strain. In addition to the usual black bees, there were also a number of yellow bees, and, in each case, the rate of dance was typical for that bee. Added to this is the fact that some intermediate forms, black bees with some yellow banding, had an intermediate rate of dancing. The possibility of misunderstandings caused by the various "dialects" in such a hybrid colony is pointed out. This study seems again to point up the correlation between inherited morphological characteristics and patterns of behavior.

Lindauer (30) has reported that bees use their ability of communicating the position of some exterior place in yet another way. When bees swarm, they usually first gather in a relatively solid clump, and scouts then take off to surrounding areas to seek a permanent home. When these scouts find such a suitable place, they return to the swarm and dance out the location of this spot. In the beginning almost every scout has a different place which it reports. In time, however, more and more agreement seems to be reached in the dances because other bees that have checked these locations indicate the most adequate one. Von Frisch's work has taught us not to be too much surprised about the behavior of bees, but this democratic procedure in an insect state seems little short of fantastic.

The orientation of allied species of ants is reported in two papers by Carthy (6, 7). In a laboratory study, *Lasius niger* was found to use the position of an electric light bulb as a means of orienting. Experiments involving polaroid filters showed that this ant uses the same plane of polarization as the bee. A related species, *Lasius fuliginosus*, pays little attention to changes in the light environment, but depends mainly on the odor trails which it lays down as guide lines.

Another area with which several European psychologists are now preoccupied concerns the ability of animals to respond to absolute direction when all local cues are eliminated. Blinded white mice were trained by Vogelberg & Krüger (67) to respond to a compass direction. A four directional, turnable apparatus eliminated apparatus cues. Six of the 18 mice learned this problem. When the apparatus was moved from place to place while situated on a moving vehicle (truck), this training broke down. Obviously some local cues still existed in spite of all precautions. However, about one-half of the animals showed a directional preference which was not related to the original training and which could not be due to chance. This is one of the studies in which an ever increasing use of statistical methods is to be seen in the work of contemporary European psychologists.

Schleidt (53), seems to substantiate this "absolute" orientation sense in homing experiments with wild mice in a field situation. Ten mice returned to their nests from distances up to 790 m. The actual time for return varied tremendously—from 10 min. to 72 hr.—and showed no relationship to the actual distance to be covered.

Experiments dealing with orientation in time have been reported by Stein (61). Working with songbirds, she used increased activity before feeding time as a measure of training. The results showed that: (a) time sense is not a hunger rhythm; (b) time sense is not determined by light and temperature; and (c) meteorological disturbances have an effect on the training. As is usual in the experiments involving time sense, it was impossible to train the birds to other than a 24-hr. cycle. An attempt at a 26-hr. cycle was unsuccessful after 31 days of training.

INSTINCTIVE BEHAVIOR

Although the term "instinct" has certainly fallen into ill repute as far as present-day psychology is concerned, it yet remains as the central problem in comparative psychology. For this reason, instinctive behavior is the major area covered in this review. Many of the other topics (sensory physiology and perception, the role of hormones and the nervous system, learning, and social behavior) fall also within the scope of other reviews in this volume.

There have been valiant attempts on the part of a few, notable among them being Lashley in his 1938 presidential address to the Eastern Psychological Association, to bring the term back into its proper place. These attempts seem to have failed. In the opinion of the reviewer the area is of extreme importance not only to comparative psychology but also to developmental, personality, and the clinical fields. The analysis of unlearned patterns of behavior, as well as their modification by experience, must remain the legitimate domain of psychology. And whether we use the term "instinct" or substitute some more sophisticated term is of little importance.

Hoarding.—In spite of this reluctance to deal with instinctive behavior, there is nevertheless at least one isolated aspect which has been given concentrated attention by American psychologists for a dozen years. This is the problem of hoarding. For the amount of effort that has been spent, however, the yield has been relatively light.

Porter *et al.* (43) attempted to determine the relation between hoarding and age. Seven groups of rats were tested, the youngest shortly after weaning, the oldest at about 320 days of age. Results showed that the number of pellets hoarded increased more or less linearly with the logarithm of age. The authors concluded that deprivation was not a necessary condition for hoarding, although the general level of hoarding activity was usually higher after deprivation than before. This study should be particularly useful when comparing the results of hoarding experiments in which rats of different ages are used.

In a more direct attempt to test Hunt's hypothesis that the effects of infantile deprivation can persist to influence adult hoarding behavior in rats, McKelvey & Marx (36) failed to get any clear substantiation. They rightly conclude that present evidence is not clear enough to justify acceptance of this hypothesis. Instead, Marx (35) offers an alternative in which he proposes that increased adult food hoarding by infantile-deprived rats may merely be the result of retention of an increased rate of eating. By eating the first few pellets returned in the hoarding tests more rapidly, the rats may have the opportunity to bring back additional pellets and learn to hoard by this method. The results showed that while there was significantly faster eating by the experimental (infantile-deprived) rats on the first day of adult deprivation, the difference disappeared by the end of the seven-day deprivation period. Marx concludes that these results provide at least preliminary support for this hypothesis. Actually there is of course very little difference between the explanations offered by Hunt and by Marx except that the latter seems to get at the more clearly defined causal factor of adult hoarding in rats. In a further paper, Marx (34) reports an experiment designed to test the influence of terminal reinforcement from feeding on the development of hoarding. Using an orthodox hoarding apparatus, Marx allowed 20 control animals to feed following the return of each of 10 food pellets placed in the bin. Twenty experimental animals were deprived of their pellets immediately upon their return to the cage. Control animals hoarded a significantly greater number of pellets on subsequent test days when 100 pellets were available to each animal than the experimental animals did. These results are, of course, not in agreement with other earlier studies in which food was removed after the hoarding period, thus allowing some feeding. Marx concludes that the development of the hoarding habit may be interpreted in terms of orthodox learning principles. Actually, this may be a logical follow-up of the hoarding work to date since results so far show little relevance to instinctive behavior patterns as they have been analyzed by Lorenz and Tinbergen. A serious attempt to investigate hoarding behavior from the viewpoint of ethological theory and method ought, however, to precede the abandonment of an instinctive behavior analysis.

The individual shyness or lack of shyness in the rat further complicates the picture. Hess (25) found that rats which took a long time to leave the home cage for exploratory behavior, and were therefore classified as the shy group, hoarded few pellets in an open (unwalled) alley situation. Nonshy rats, on the other hand, hoarded a great deal of food. When the orthodox, walled alley was used, this situation was reversed, so that now the shy rats hoarded more food. An intermediate type of alley, one with walls and top of wire mesh, gave results somewhere between the two other cases. This suggests a substantiation of the "security hypothesis" previously proposed in the hoarding literature, but does little to clarify the total problem.

Several papers dealt with physiological alterations of the hoarding animals and the subsequent effect on hoarding behavior. Stellar (63) investi-

gated the effect of metabolic change. Depression of metabolic activity was accomplished by thyroidectomy and by thiouracil administration, while injection of thyroxine elevated metabolism. Although physiological indices showed up the differential treatment, no significant differences in hoarding activity were observed. Stellar also stresses the need for additional facts about hoarding and less theorizing. This is excellent advice. Above all, it would seem useful to study hoarding behavior in other species, particularly some which have not been so completely domesticated as the laboratory rat. Williams *et al.* (69) reported the effect of a series of electroconvulsive shocks (ECS) on hoarding in rats. During the first 12 days after termination of shock, the ECS group hoarded significantly fewer pellets. This difference diminished progressively during the second period of 12 days. Since the general activity level of the post-ECS rats was relatively low for the first 12 days, these results are not entirely surprising. Finally, Zubek (71) reported a study to determine the role of the cerebral cortex in hoarding. He found that, on the whole, lesions anywhere in the cortex increase hoarding. This is rather surprising, since similar lesions decrease copulatory and maternal behavior. One way out is to postulate hoarding to be more primitive than either copulatory or hoarding behavior and that the cortex serves only to inhibit hoarding. It would have been instructive had Zubek recorded activity levels for both groups of rats, since the effect may have been due to general activity level as was probably the case in the previously mentioned experiment.

Although we have had a great many experiments on the problem of hoarding in which environmental, psychological, and physiological variables have been investigated, we seem to have made relatively little progress. Perhaps new methods and new animal material would help.

Mating and sexual behavior.—Young & Grunt (70) published an account of sexual behavior in the male guinea pig in which they described a method for the measurement of drive. A score or quotient was derived, using 10-min. observation periods, which seems a promising tool in the study of sexual behavior in other mammals as well.

Several papers dealt with mating behavior in species ranging from insects to mammals. Spieth (60) discussed mating behavior in the *Drosophila virilis* species. Unfortunately the mating pattern in all races studied is so similar that there seems little hope of using this available material for genetic studies involving inheritance of instinctive behavior patterns. The same difficulty was encountered by Prechtl (46), who reported on mating behavior in newts. Some differences, particularly in the frequency and amplitude of tail movements during courtship, do offer a possibility of genetic study since many of the newts are readily hybridized. An analysis of the reproduction of the squirrel is reported by one of Lorenz's students (13) using his typical analysis of behavior and behavior inventory.

Beach (3) studied the effects of forebrain injury on mating behavior in male pigeons. Observing a variety of instinctive movements, he found that the same lesion affected the behavior components in a differential way. Suc-

cessful copulation dropped out of the behavior. The administration of androgen, interestingly enough, had an effect similar to that found in androgen-treated castrates; copulation was again successfully demonstrated.

Migration.—Two small books summarizing some of the known facts of migration appeared. The first is by Lincoln (29) and seems to be little more than a sketchily revised version of the 1935 publication by the same author. It is nicely set up, however, and provides a quick look at some of the facts of migration. Similar in many respects is the German publication by Steinbacher (62). His manuscript, completed in 1943, lay fallow during the post-war years, but apparently some additions were made to the text so as to include some of the more recent work. While the book is not written for narrow scientific circles, it does present some interesting views, particularly of the European methodology in this field. The section on orientation during migratory flight (pp. 127-47) is especially informative. Both books are well illustrated.

The single study to be mentioned here is one reported by Odum & Perkinson (40). It deals with the relation of lipid metabolism to migration in birds. The authors found that a necessary prerequisite for migration is lipid deposition and that birds will migrate only when this process is complete. Their conclusion, that "much concerning the mechanics of migration may be learned from a study of the lipid levels in a population" leaves us with the problem of how to go about using this sort of technique. It is something like the work on photoperiodicity, which, while it sheds some light on a variable, is scarcely a generally useful tool.

Maturation and learning.—In a study paralleling those in which chimpanzees are subjected to limited visual experience, Nissen *et al.* (38) reported the effects of restricted opportunity for tactual, kinesthetic, and manipulative experience on the behavior of a chimpanzee. The limbs of a four-week old male chimpanzee were encased in cardboard cylinders for 30 months. Normal infants of the same age were available for comparison. Results showed fairly normal behavior except for an atypical sitting posture, lack of grasping the attendant who carried him, and difficulty in learning a problem which required discrimination of tactual-pressure stimulation at two separate points. This latter task took 10 times longer than for a comparable control animal. Visual discrimination habits, as might be expected, were learned easily. The authors conclude that, though only one animal was used, the experiment indicates the general plan to be feasible and the study points out those areas in which experience is important for integration.

Poulsen (44) reported that chicks, kept and fed four days in the dark, pecked at all sorts of objects when released in a lighted environment. After only 4 hr. of pecking experience the chicks pecked solely at food objects. In the same paper, Poulsen also discussed flight movements of young swallows and concluded that maturation alone was of importance for the development of flying in this species. In another paper, Poulsen (45) showed that the

length and general rhythm of the chaffinch's song were innate but that the song proper needed to be learned from species members.

Innate and learned enemy recognition in the bullfinch was studied by Kramer & St. Paul (28). A number of objects were presented to eight young birds and fright reactions to these objects were then recorded. That it was not the species but rather the size to which fright responses were directed was shown by using 15 stuffed birds ranging in size from wren to grouse. With balls of rabbit fur, owl feathers, cloth, as well as other neutral objects (stones, etc.), more tests were run. Neutral objects released fright reactions in 12.5 per cent of the presentations; with the fur ball, the numbers rose to 53 per cent and were highest with the feather ball when 72 per cent fright reactions were released. Kramer & St. Paul concluded that color and texture of enemies (birds of prey) were innately recognized but that form was probably learned. This latter conclusion was based on experiments with plaster models of various birds.

Endogenous movements and releasers.—For explanations of the terms used in this section the reader is referred to Tinbergen (66) or to the paper by Nissen & Semmes (39) in this review series (pp. 234–36).

Peiper (41) pointed out that the sucking movements of infants can be considered to be endogenous movements as they have been defined by Lorenz. Chiefly characteristic is its appearance in *Leerlauf*, i.e., without stimulation. In another paper, Precht & Schleidt (47) published a continuation of comparative studies involving the releasing and taxic components of the sucking mechanism. Cats, dogs, rabbits, hares, and hamsters were used. The actual shape of the teat was reported to influence sucking. When the teat was short in form, pumping movements were observed; when long, licking movements predominated. In the cat, rat, hamster, and man where pumping movements of sucking are to be found, there was also *Leerlauf*. In the dog and rabbit where licking movements are the rule, this "unstimulated" response was not found.

A study of the visual releasers, which cause male butterflies of the *Pieris* genus to make their first flight to females, was made by Petersen *et al.* (42). The behavior of *Pieris napi* and *Pieris bryoniae* was reported. Using models, it was found that the white components of the wings had great drawing power, while yellow and the dark patterning, usually found on the wings, had little effect. Painted paper models varied greatly in effectiveness. Those painted with zinc white were approached, those with lead white were not. The authors advanced the notion that the high ultraviolet reflection of zinc white could serve for an explanation of these results.

Auditory releasers in field crickets were studied by Weih (68). Normal stimulation was relatively flexible in both quality and rhythm, while responses to male rivals followed a strict metric.

General ethological studies.—There are a large number of papers, particularly in the biological journals, which deal with life histories carried out

within a general ethological framework. Some, which are of wider interest to comparative psychologists, are briefly mentioned below.

A detailed, well-illustrated study of cockroach behavior was published by Roth & Willis (50). The huge bibliography alone should make it a useful reference. Another insect problem, in which the visual capacity of dragonflies (*Calopteryx*) was studied in an ethological framework, is discussed by Buchholtz (5).

While the ornithological literature is filled with a variety of material of interest to comparative psychology, it would fall well outside the scope of this review to begin either an evaluation or even a listing of this body of information. A representative study, published in a journal of psychology, is the paper by Koenig (27). It presents an account of a flycatcher (*Merops apiaster*) species in which most ethological problems are considered.

Brief, but useful and representative, material dealing with the ethological study of mammalian behavior may be found in the German publication *Zeitschrift für Tierpsychologie* (12, 20, 31, 33, 51, 52, 54, 57).

LEARNING

Most of the reported studies which deal with learning in animals are covered in the chapter on learning, but it seems useful to consider a few of them which are more interesting for the comparative approach.

Gelber (21) has definitely come to answer the plea of those who ask for experiments dealing with organisms other than the laboratory rat. Her experiment is an attempt to show learning in *Paramecium aurelia*, although she herself prefers the more cautious term "modification of behavior." Training trials consisted of lowering a platinum wire into a homozygous culture of about 128 experimental animals. Reinforcement consisted of a very small amount of food which was wiped onto the wire. Tests for learning were made by counting the number of animals which adhered to the sides of a sterile wire after completion of the experiment. Differences for reinforced and non-reinforced animals were statistically significant at the .01 level of confidence.

The formation of a conditioned response in spinal frogs was reported by Franzisket (18). Wiping responses, made by the frog when stimulated with thin wire, were observed in the experiment. By simultaneously stimulating two points on the body of the frog, one with a scratch stimulus twice as strong as the other, Franzisket found that the frog would respond only to the stronger of the two stimuli. After 100 to 350 trials, the weaker, or conditioned stimulus, used alone would cause a reaction in the other reflexogenous zone which had previously been stimulated with stronger pressure. An example may make this clear. A weak scratch on the flank coupled with a simultaneous, but stronger, scratch on the arm was answered only by arm wiping movements. After 325 such combinations a series of five stimuli on the flank, presented alone, resulted in wiping movements of the arm. Since under ordinary conditions a flank stimulus results in a foot movement to that area,

Franzisket feels this to be a successful demonstration of spinal conditioning. Histological evidence rounds out the report.

Two papers dealt with learning in chickens. Altevogt (1) investigated differential learning capacity in several breeds and found that the smaller varieties (Bankivas and Bantams) grasped the visual discrimination problem in a shorter time than did the largest breed used (Brahmas). Leghorns, which were used as the "normal" group, were intermediate in both physical size and learning performance. In learning a series of discriminations, however, the heavy Brahmas caught up with the lighter breeds by the third problem. While the heavy breeds took more time to grasp the problem at first, they readily surpassed the quicker, small breeds when a more complex series was presented. Altevogt uses these experiments as substantiation of Lashley's cortical "mass-action" since the larger breeds also have a larger absolute brain size. Although these experiments have an unfortunate resemblance to Jaensch's "barnyard personality studies," they do seem to point up differences in body and brain size within a species which correlate with learning capacity. Whether this has any real bearing on human psychology is of course an open question. The second paper is one by Engelmann (15). From experiments and observations he concludes that chickens retain longest the memory of food, less long the memory of places. Memory tests also work better in trials with single animals than when groups are used.

An important paper in the field of bird learning is Thorpe's review (65). It covers material dealing with habituation, imprinting, trial-and-error, imitation, conditioning, and insight learning. By habituation Thorpe means "the simplest kind of learning . . . a simple learning *not* to respond to stimuli which tend to be without significance in the life of the organism." Habituation as a learning process can thus readily be demonstrated in all animals from protozoa to man. The other, perhaps less familiar, term is "imprinting." This is the concept originated by Lorenz. Thorpe does not, however, clearly define imprinting in his paper, although he does touch on some of the characteristics which have been set down by Lorenz, the most important being: (a) it has extreme stability and (b) it is mostly present in a very definite and very brief period in the early part of an organism's life. The paper includes a thorough bibliography and the statement that the subject of imprinting "needs, and would repay, full and precise experimental investigation almost more than any other aspect of animal behavior." This is scarcely an overstatement when one considers the implications this sort of rapid and early learning may have for personality theory and general clinical psychology.

While experimental evidence for imprinting is slow in accumulating, two excellent papers have been published. The first is a report by Ramsay (49). Although the work is directed to the ornithological problem of familial recognition, it contains an account of incubator-hatched birds and their response, or imprinting, to inanimate objects. These objects were present in the incubator at the time the birds hatched and were intended to be substitutes for the parents. Tests were then run to see whether the birds would follow these

objects, as they ordinarily follow the parent, in a field situation. A green box, containing a ticking alarm clock, was followed by chicks and Canada goslings when moved just above the ground on a wire support. Muscovy ducklings and goslings responded to a football while young mallards responded to neither of these objects. Fabricius (17), in a comprehensive monograph, reports a study of imprinting in which he used young tufted ducks, shovellers, eiders, and mallards. Hatching eggs were placed in an incubator and covered with paper hoods. The ducklings were then brought out at different ages and confronted with the object on which they were to be imprinted. All of the work was done under well-controlled conditions and the results were as follows: (a) Ducklings were imprinted as successfully as were gray geese (the original Lorenz experiment). (b) The imprinting proved to be irreversible, thus fulfilling one of the postulated characteristics of the process. (c) The size of the object could vary within wide limits—from ducklings twice the weight of the newly hatched one, to man. Ducklings did not become imprinted on brothers or sisters. (d) Acoustic signals were effective but did not have to resemble accurately the quacking of a duck. The author used the call "kom kom kom." (e) The sensitive period for imprinting was strongest in the first 6 hr. after hatching and had practically disappeared at the age of 24 hr.

It would seem, on the whole, that an important problem in this area would be an extension of experiments to see whether or not this process can be demonstrated in mammals. Unfortunately, at least at present, only students of bird behavior appear to be sufficiently interested in this aspect of learning to carry out the painstaking experiments and these investigators are not likely to turn to animals other than birds.

SOCIAL BEHAVIOR

Four papers dealing with social behavior in birds form part of a symposium on that subject. Collias (9) has given an admirable account of the development of social behavior in birds. It is bolstered with much original material. There is a good account of prehatching behavior followed by a consideration of initial social adjustments and responses after hatching. Using such sophisticated material as sound spectrograms of chick vocalization, he presents original experimental evidence of early social behavior in chicks. Tests for early "social plasticity" were made on chicks one, two, four, six, eight, and ten days of age by observing individual following responses to clucking. Similar tests using a large retreating object were also made. The results show a decreasing effectiveness of both of these stimuli as the chicks grow older. While the experiments dealing with response to clucking seem adequately designed and controlled, the method involving responses to a large retreating object appears to leave something to be desired. The actual method was as follows:

The observer placed a chick on the floor and then placed himself directly in front of and facing away from the chick. The observer then proceeded slowly away from

the chick, often stopping momentarily, or even backing up a few steps, this being continued until about 10 feet had been covered.

It would perhaps have been better to use a uniformly moving object which could be mechanically moved in a repeatable manner. Nevertheless the results of these two tests are sufficiently in agreement. Other topics covered by Collias are: appearance of new social responses, the role of leadership in socialization, and the development of sexual and parental bonds.

Of less interest, perhaps, to the comparative psychologist is the report on flocking behavior in birds given by Emlen (14). Social, physiological, and other environmental factors are discussed. Davis (11) deals with social behavior and reproduction and restricts his presentation to those aspects of social behavior that influence the reproduction of the species as a population. In the final of the four papers Darling (10) discusses social behavior and survival. After presenting some evidence, dealing with a variety of animals, Darling properly stresses the need for further research on the whole question of social behavior and its survival value for the individual and the species.

Altmann (2), in a research project carried over four summers, dealt with the social behavior of elk in the Jackson Hole area of Wyoming. She reports some changes in group structure, a regular diurnal schedule of action, and firmly established dominance-subordinate behavior in this species.

Several studies dealing with aggressive behavior in mice were published. The most impressive of these was the report of Scott & Fredericson (55). After reviewing some of the results of study in aggressive behavior, carried out mainly with mice, rats, and goats, Scott & Fredericson conclude that "frustration as a cause of aggression is important only when animals have been trained to react aggressively after frustration." Both hereditary (sex) and environmental factors (protection or lack of protection in early training) were considered. Of special interest to the psychologist may be the "principles" or laws of learning (pp. 286-87). They include: (a) adjustment or adaptation; (b) primary stimulation or releasers; (c) association, reinforcement (memory); (d) negative association (inhibition, extinction); (e) strength of association (memory); and (f) generalization and discrimination.

Smith & Ross (59) reported the effect of avitaminosis on the social dominance in male C_3H mice. Eight groups of three C_3H mice were subjected to a vitamin-free diet after the dominant mouse in each group was identified. Some cannibalism was observed and the dominant animal of each group survived the other members. Fighting practically ceased after four days of vitamin-free diet and by the seventeenth day 22 out of the 24 animals were dead.

With the hypothesis that early traumatic experience has a greater effect, Kahn (26) studied the effect of severe defeat at various age levels on the aggressive behavior of mice. His hypothesis was substantiated, and Kahn observed, in addition, that trained fighters become more aggressive with practice.

In the final paper, Rabe (48) reported an experiment to demonstrate the

cumulative frustration effect in C_3H mice. Using rate of defecation as the criterion for frustration severity, Rabe placed the experimental animals under glass beakers for a three minute "frustration" period. The animals were then placed in an open field (an open platform 50 cm. wide, without walls), for an added 3 min. Rabe seemed to recognize the limitations of his control procedures, which consisted of placing animals on 6×6 cm. wooden platforms and an elevated triangular runway, but concluded that the experiment showed the positive effect of cumulative frustration.

Although comparative psychology is scarcely in full flower, neither does it deserve the obituaries of recent years. Maybe current European work will turn out to be just the needed shot in the arm. Perhaps then comparative psychology will again take its rightful place as one of the foundations on which most psychologies need to be built.

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PHYSIOLOGICAL PSYCHOLOGY^{1,2}

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If we judge in terms of published reports, the fields of sensory discrimination, learning, and emotion are of primary interest to the psychologists and physiologists who do research in the area called physiological psychology. In all three of these fields, new psychophysiological experiments have been stimulated by the recent findings of neurophysiology and neuroanatomy.

In other fields, such as the study of basic drives and of brain function in disease, one can see the beginnings of a relationship between biochemistry and psychology similar to that which has existed in the past between psychology and physiology. The rate at which this relationship develops will depend upon the physiological psychologist's ability to communicate with his colleagues in biochemistry.

SENSORY DISCRIMINATION

Hearing.—The least explored part of the auditory nervous system is that which includes the centers and pathways from the primary cochlear nuclei in the medulla to the medial geniculate bodies in the thalamus. New and more complete maps of this territory have been made. Thurlow *et al.* (99) and Gross & Thurlow (37) have used microelectrodes to study electrical responses of the inferior colliculi and medial geniculate bodies to auditory stimuli. Responses were obtained to frequency "sweeps," clicks, continuous tones, and thermal noise. Response areas indicating some frequency specificity were found for both the inferior colliculus and medial geniculate; these response areas were similar to those reported by Galambos & Davis (35) for the primary auditory nucleus. Average latency of spike responses was 7 to 8 msec. at the inferior colliculus and 9 to 10 msec. at the geniculate. In both centers, spike discharge increase with intensity up to a maximum of about 100 impulses per second. Rapid adaptation to a steady rate was found. Masking phenomena were also noted.

In a preliminary report, Galambos (34) has described the spike responses recorded with microelectrodes in the medial geniculate body in response to click stimuli. He found that latency of response ranged from 6 to 125 msec., the latency of a given unit varying by as much as ± 10 per cent of its median value. Galambos interprets the wide range of latencies as being due to variation in neural pathways from the ear to geniculate. Erulkar (29)

¹ The survey of the literature to which this review pertains was concluded in June, 1952.

² The following abbreviations are used in this chapter: MES (middle ectosylvian); cff (critical fusion frequency); EEG (electroencephalogram); TEA (tetraethylammonium).

has recorded the electrical responses evoked by click stimuli from the mesencephalon of the pigeon. A monopolar electrode was used and its placement was determined by post-mortem histological examination. Potential changes were found in the principal magnocellular and principal parvocellular divisions of the nucleus isthmi. Latency of response was about 4 msec. This latency when compared with that obtained from higher centers led Erulkar to conclude that parts of the nucleus isthmi form a major synaptic region in the direct ascending pathway of the auditory system.

Evidence from the study of the brain stem reticular system by Starzl, Taylor & Magoun (85) indicates that collateral afferents of the ascending auditory tracts feed into the reticular formation; these collaterals apparently are present at levels below the inferior colliculi and as far forward as the posterior part of the thalamus.

The electrical responses of the auditory projection areas of the cortex have been recorded in a number of studies using different kinds of auditory stimuli and different techniques of recording. Lilly & Cherry (58, 59) have recorded patterns of spontaneous activity and of sound-evoked responses from an array of 25 electrodes placed on the auditory cortex of the cat. On the basis of characteristics of the patterns recorded, Lilly & Cherry conclude that the auditory area can be divided into three regions corresponding to those described by Rose & Woolsey, i.e., AI, AII, and Ep. Hind (42) has also mapped the auditory cortex of the cat, recording strychnine spikes evoked by tonal pulses. On the basis of the tonotopic organization which he described, the subdivision of auditory cortex into AI, AII, and Ep areas also seems plausible.

Tunturi (100) has reported a further analysis of the electrical responses elicited from strychninized patches of the MES² region of the dog's auditory cortex. In earlier experiments he found a tonotopic organization in the MES region, and a given vertical band of cortex was most sensitive to a narrow range of frequencies. In the present experiment he has recorded responses along the bands to different intensities of sounds. When the sound was presented to the contralateral ear, the response curves for different points along the band remained the same. When the stimulus was given at the ipsilateral ear, however, the minimal intensity necessary to elicit a response increased as the place of recording was moved downward. A gradient of about 5.9 db. per mm. was found.

Rosenzweig (82) recorded the responses obtained with gross electrodes on the cortex of the anesthetized cat when click stimuli were presented to one ear then the other. He found that, in the majority of cases tested, the response obtained from either hemisphere was significantly larger for contralateral than for ipsilateral stimulation. The response obtained when both ears were stimulated simultaneously was larger than that for either ear alone. When the two ears were stimulated successively with the time interval under 50 msec., interaction occurred, the response to the second stimulus being reduced. Rosenzweig concludes that the two ears are represented at the

cortex by two overlapping populations of neural units, the population for the contralateral ear being greater.

Gellhorn (36) has shown that anoxia reduces the potential elicited by auditory and visual stimuli from their respective cortical projection areas. Under the conditions of his experiment, which did not provide for very exact control of the stimuli, no significant differences was found in the sensitivity of acoustic and optic projection areas to anoxia.

Chow (24), by anatomical methods, has estimated the number of cells in the different nuclei of the auditory system of the monkey (*Macaca mulatta*). There is increase in the number of cells from approximately 88,000 in the primary cochlear nucleus to nearly 400,000 in the inferior colliculus and in the medial geniculate and to about 10,000,000 in the auditory cortex.

Many of the current studies of behavioral changes after lesions or ablations of central nervous system structures are being guided by the results obtained by electrophysiological methods. The auditory projection areas of the cortex of the cat, dog, and monkey have been redefined by the evoked potential method. In addition subdivisions of the projection areas have been described, e.g., the AI, AII, and Ep areas of cat cortex mentioned above. The tonotopic organization of the auditory cortex has led to the hypothesis that the auditory cortex plays an important role in discrimination of changes in frequency. Experiments by Meyer & Woolsey (63) and Butler & Neff (20) agree that in the cat, at least, small differences in frequency can be discriminated after bilateral ablation of auditory projection areas AI, AII, and Ep. Meyer and Woolsey report impairment of frequency discrimination but good intensity discrimination when somatic area III, in addition to the auditory areas, is ablated bilaterally.

Diamond & Neff (27) report impairment in the ability of cats to discriminate changes in temporal patterns of tones after bilateral ablation of auditory areas AI, AII, and Ep. Bilateral ablation of AI alone produced little or no effect. Neff (70) has emphasized the need for psychophysiological studies to supplement the neuroanatomical and neurophysiological evidence which has, in the past, formed the primary basis for theories of hearing. The studies of frequency discrimination (20) and localization of sound in space (71) are cited to illustrate the point that neuroanatomical and neurophysiological evidence may not be sufficient for accurate predictions as to the role of the auditory nervous system in discriminatory behavior.

In a study, which was concerned mainly with tracing of anatomical connections, Lemmen (56) reported some limited observations which indicate that in the monkey no serious impairment of a learned auditory discrimination occurs after unilateral ablation of the cortex of the superior temporal gyrus adjacent to the primary auditory area.

Vision.—Bishop & Clare (17) have questioned the conclusions of Chang & Kaada (23) that the different components which make up the pattern of electrical response of optic cortex to optic nerve stimulation represents the activities of a triple conduction pathway in the visual system. On the basis

of records obtained from electrodes placed at various depths in the optic cortex and arranged so that leads could be taken from different electrode combinations. Bishop & Clare assert that

of the successive spikes up to five which can be recorded at the cortex in response to single shock at the eye socket, all but the first one may be concluded to represent groups of neurones firing within the cortex after successive synaptic passages.

The fact that multiple spikes can be recorded from the white matter after removal of the optic cortex is offered by Chang (21) as an argument against Bishop's interpretation. Chang (22) has also presented evidence that the triple conducting pathway postulated by him and Kaada is related to trichromatic vision. He was able to do this by utilizing a potentiation effect, in which cortical responses elicited by stimulation of the lateral geniculate are enhanced when the retina is exposed to light. Illumination with monochromatic lights gave differential potentiation.

In a more detailed study of the potentiation effects, Chang (21) reports that when the eye is continuously stimulated by light, the threshold of cortical response to lateral geniculate stimulation is lowered and the size of the response to suprathreshold stimuli is increased. Spontaneous activity is depressed. Chang concludes that the site of the potentiation is in the lateral geniculate and he speculates on the relation of these results to problems such as maintenance of alertness due to constant sensory inflow and the interaction of different sensory systems.

In connection with the different interpretations offered by Bishop and by Chang of evoked cortical response to visual stimulation, attention should be called to Eccles' more general discussion of action potentials in the cerebral cortex (28). Eccles suggests that the cortical potentials in response to afferent volleys represent the synaptic potentials generated in afferent neurones ending on the long vertical dendrites of pyramidal cells. The potentials which often follow the initial potential may be similarly explained as resulting from impulses after one or more synaptic relays in the cortex.

During the past year there have been no published reports of experiments in which visual discrimination was studied after ablations of the visual projection areas. There have been, however, a number of studies relating visual discrimination to ablation of "association" areas or to accidental cerebral lesions.

Chow (25) examined the effects of prestriate and temporal neocortex and frontal eye-field lesions on a variety of visual discriminations in the monkey. He found no impairment of the primary visual capacities: visual acuity, visual field, visual pursuit, and depth perception. After extensive prestriate lesions, temporary loss in object recognition and general orientation was noticed. A deficit in ability to discriminate differences in pattern and color found in one animal with prestriate ablation was attributed to the restricted visual field of the animal. The most severe postoperative deficit occurred in an animal with bilateral ablation of temporal neocortex. Object

recognition was temporarily deficient and relearning of brightness, color, and pattern discriminations took longer than original preoperative learning. Ablation of the frontal eye-fields had no noticeable effect on visual behavior. The author points out that these results, which agree with those of earlier experiments, are difficult to interpret until additional information is available on the intercortical and cortico-thalamo-cortical connections of the striate, prestriate, and temporal areas.

Battersby (11) compared cff,² i.e., the minimal rate of light intermittence at which subjective fusion occurs for (a) normal subjects, (b) patients with frontal lobe injury, and (c) patients with hemianopsia and evidence of damage to the geniculo-calcarine system. Using targets of .5° and 2° visual angle, he measured cff at the fovea and at different distances from the fovea in both nasal and temporal fields. For all groups, the cff was found to decrease with angular distance from the fovea. For the normal and frontal lobe groups cff was greater in the temporal than in the nasal field. Definite decrease of cff was found even when the light stimulus was confined to the perimetrically normal visual field. No depression of cff was found in the group with frontal lobe damage. Five of 10 patients with hemianopsia could perceive and localize a light stimulus presented in the perimetrically blind field.

In a related study Battersby, Bender & Teuber (12) compared the cff of normal and frontal lobe damaged subjects, the area and intensity of the light source being systematically varied. The cff's of the frontal lobe group were slightly lower than those of normals but the difference was significant only for a very small target (38') at a high intensity level (3.61 e.f.c. per sq. ft.). With both normal and pathological groups, cff was found to increase approximately linearly with increase in log target area; with total light flux constant, it increased with decrease in target area.

Teuber, Battersby & Bender (96) have also compared different groups of brain-injured war veterans on complex visual tasks. The patients were divided into three groups, according to the estimated damage as being to the anterior, intermediate, or posterior areas of the brain. A normal cortical group was also given the battery of visual tests which included the Gottschaldt figures, modified Wisconsin sorting test, visual choice reaction, and matching from sample and oddity problems. Some impairment on the visual tasks was found for all the brain-damaged groups; the posterior and frontal lobe groups showed the most severe deficits. (For further studies involving visual discrimination, see section on LEARNING, MEMORY, AND PROBLEM SOLVING.)

BASIC DRIVES

Hunger.—The year 1951 was an eventful one for "learning theorists"; they rediscovered the stomach. Using a dog with an esophageal fistula, Hull *et al.* (44) found that real feeding was more strongly reinforcing in a learning situation than sham-feeding and that a learned habit could be established on the basis of esophageal feeding.

Kohn (50) used rate of performance of an instrumental response to measure hunger drive of rats with stomach fistulas. A comparison was made of the short term effects on hunger drive of (a) milk injected directly into stomach, (b) milk taken by mouth, (c) normal saline injected into stomach. It was found that hunger drive was reduced by both stomach and mouth feeding, the latter leading to the greater reduction. The author concludes that "drive reduction can occur in the absence of need reduction" since the effects on the instrumental response "appeared before digestion and absorption could have produced significant changes in the blood or tissues." Miller, Kessen & Kohn (66) have reported that rats with stomach fistulas can learn to chose the alley of a Y-maze in which they receive milk by stomach tube as opposed to the other alley in which they receive an equal amount of normal saline.

Janowitz & Grossman (46) compared the average daily volume of food intake for control animals and for experimental animals fed sucrose solution, cream, solution of casein hydrolysate, alcohol, or bitters 20 minutes before regular feeding. Small amounts (e.g., 50 to 100 cc.) of the above solutions did not change significantly the amount of regular food intake. Sucrose solution or cream in amounts of 300 to 400 cc. had a slight effect on food intake; this depression, the authors believe, was due to gastric distention. Since the small amounts of sucrose and cream were sufficient to cause endogenous release of enterogastrone, it appears that the latter does not contribute directly to satiety.

McCleary (61) has investigated the physiological mechanisms which underly the rat's preference for different solutions of sugar. He has found that preference, measured in terms of choice of several solutions presented simultaneously, is not the same as preference measured according to amount ingested of solutions presented singly. McCleary concludes that one important physiological factor in determining the amount of a solution a rat will ingest is the osmotic effect the solution has when it reaches the stomach. The osmotic effect probably influences the animals' water and electrolyte balance and the latter, in turn, affect the taste receptors and central nervous system to control the intake of the sugar solution.

Rats will become alcoholics according to Rosenberg & Zarrow (81) if given appropriate amounts of propyl thiouracil. Alcoholism was measured by the ratio of water intake to 10 per cent alcohol intake in a free choice situation. The ratio varied from 2.71 for normal animals to as low as .28 for animals receiving 0.4 per cent concentration of propyl thiouracil in their diet. The "alcoholism" was not affected by administration of thyroxine and could not be produced by thyroidectomy. It disappeared upon cessation of thiouracil treatment.

The salt preference of the rat has been investigated by Wiener & Stellar (103); rate of drinking and amount drunk were both measured after a 15-hr. water deprivation period. The single stimulus method was used, i.e., only one bottle containing a given solution was presented during any one test

period. Among the results obtained were these: (a) The threshold for the rat for salt solution is at least as low as 0.05 per cent, the point of maximum preference is about 0.8 per cent, and the aversion point, 1.5 per cent; (b) salt preferences are apparent almost immediately when the rat starts drinking, this promptness of appearance making questionable any explanation in terms of the physiological effects of salt absorbed in the gut; and (c) salt preference appears promptly in the naive animal and does not depend on previous experience.

Thirst.—In two studies (92, 106), apparatus for getting continuous records of rate of drinking has been described. Young & Richey (106) found that rats show a diurnal drinking rhythm; the rate of drinking was higher at night than during the day. Saccharine-sweetened water was preferred to distilled water. Stellar & Hill (92) likewise noted a diurnal rhythm in the drinking of rats; 78 per cent of drinking occurred at night. In addition they studied rate and amount of drinking after deprivation periods ranging from 6 hr. to 7 days. No matter what the length of deprivation, when a rat drinks, it drinks at a constant rate,—about .03 cc. of water per second. Total intake in a given postdeprivation period is a negatively accelerated function of length of deprivation time. Young, Heyer & Richey (105) compared drinking behavior of rats after water deprivation and after dehydration produced by subcutaneous injection of NaCl. Drinking patterns for the two conditions were different. In a 1-hr. drinking period after 23-hr. water deprivation, rats drank for 10 to 15 min., then stopped. After subcutaneous sodium chloride injection, there was a latent period of about 8.8 min. before drinking started; drinking continued intermittently throughout the 1-hr. postdeprivation test period. Stellar (91) has reported that injection of water or salt solutions into the stomach of rats through a stomach tube had an almost immediate effect upon drinking behavior. Injection of hypertonic salt solution increased consumption of hypotonic solutions and vice versa.

In a number of experiments with both human subjects and animals, Holmes & Montgomery (43) found that increased drinking did not occur after hemorrhage unless the hemorrhage was accompanied by more complicated metabolic and circulatory phenomena of shock. Montgomery & Holmes (68) have also shown that drinking after intravenous 20 per cent NaCl injection can be inhibited by mechanical distention of the stomach or by water given per rectum.

Sex.—Beach (14) has reported that pigeons with forebrain ablations show a decrease in copulatory activity. If the ablation is not too extensive, copulation may survive postoperatively or may be reactivated by administration of androgen. It is suggested that the "avian forebrain, like the cerebral cortex of lower mammals may act as a facilitating agent in masculine sexual behavior." Thomson (98), in a study of the effects of light on the oestrus cycle in the ferret, has found that the retina and optic nervous system are essential parts of the mechanism whereby increased light affects the hypophysis and eventually the gonadal response.

EMOTION

Anatomical and electrophysiological studies.—In his chapter on emotion in the *Handbook of Experimental Psychology* (92a), Lindsley reviewed the research done prior to 1950 on the reticular formation and the diffuse thalamo-cortical projection system; the roles of these parts of the central nervous system were emphasized in an "activation theory of emotion." New information on the interrelations of the reticular formation, thalamic centers, and cerebral cortex is coming in a steady flow from the current researches of neurophysiologists and neuroanatomists.

MacLardy (60), after reviewing anatomical evidence, considers it unlikely that there are nonspecific, diffuse, cortical projection centers in the thalamus, e.g., the intralaminar nuclei suggested by Morison & Dempsey (69) and by Jasper (47). Instead, MacLardy feels that the evidence

favors the concept of some intrathalamic diffusion system from one center to many of the main thalamic nuclei, thus permitting coordinated widespread activation, through these and their recognised specific projection pathways, of all neocortex except perhaps some of the temporal lobe.

Fortuyn & Stefens (32) have also questioned the likelihood of direct diffuse projection to cortex from some of the thalamic nuclei that have been suggested. They cite evidence that the intralaminar and midline nuclei project to the caudate nucleus and that the centromedian nucleus projects to the putamen and claustrum.

From results of electrophysiological experiments, Starzl, Taylor & Magoun (84) have concluded that, in the cat, the reticular activating influence may be mediated by two routes: (a) via the centromedial part of the thalamus from which increasingly diffuse transmission to dorsal and lateral thalamic structures apparently occurs, and (b) via an extrathalamic path involving sub- and hypothalamus and the internal capsule.

Hanbery & Jasper (38) investigated the diffuse thalamo-cortical system in the anesthetized cat by electrophysiological methods and concluded that the diffuse thalamo-cortical projection system does not act via specific thalamic nuclei but has independent projections to the same cortical areas (including sensory) as are supplied by more concentrated projections from specific thalamic nuclei.

Records of electrical activity have been obtained by Henry & Scoville (40) from the cortex of human subjects during frontal lobe operations. One of the important findings of the study was that suppression-burst activity occurred in frontal cortex isolated by undercutting. This activity, which was still present upon reoperation in some cases, is apparently independent of thalamo-cortical circuits. The experimenters suggest that it may be related to release of the cortex from the reticular activating system.

Behavioral studies.—After placing lesions stereotactically in the medio-dorsal thalamus of cats, marked changes in emotional behavior were observed by Schreiner *et al.* (87). There was reduction in spontaneous activity, but

noxious stimuli and ordinary visual, tactual, and auditory stimuli produced exaggerated irritability, "rage," and fighting responses. In some cases also an increase in "pleasurable" responses was noted. Schreiner, Kling & Galambos (86) have reported the following behavioral changes in cats: (a) after destruction of ventromedial hypothalamic nuclei, development of pronounced savageness; (b) after destruction of anterior thalamic nuclei, diminution of aggressive behavior; (c) after lesions of thalamic laminar nuclei, transient lethargy and somnolence but retention of "anger" responses; (d) after large rostradorsal midbrain lesions, loss of faciovocal activity and "rage," but no loss of withdrawal and escape behavior; (e) after lesions of midbrain tegmentum, somnolence and loss of reactivity to noxious stimuli.

Lesions in the vicinity of the mammillo-thalamic tract and adjacent structures of the diencephalon of the cat result in akinetic and apathetic states, according to results obtained in experiments by Meyer & Hunter (64). With some lesions, changes were found in the EEG² similar to those which occur during sleep; cortical response to sensory stimuli was diminished.

Akert, Koella & Hess (3) have induced sleep in the cat by electrical stimulation of lamina medullaris interna of the thalamus. Onset of sleep was accompanied by interruption of the activation pattern and the appearance of an 8 to 12 per sec. rhythm.

Several reports of clinical observations on patients are closely related to the electrophysiological and ablation studies. Concerned with the neurophysiological basis of alertness, wakefulness, and emotional reactivity, Meyer (65) has criticized Dandy's striatal theory of "the center of consciousness" and cites evidence from 24 cases in which surgical removal of parts of the anterior striatum failed to produce any prolonged period of "unconsciousness." Jefferson (48) has presented the case histories of six patients with lesions involving the mesencephalic tegmentum. Apathy, exaggerated sleepiness, and coma were symptoms noted. Jefferson suggests disruption of function of the reticular activating system as a cause of the symptoms.

Nielsen (72) has described a case of akinetic mutism in which the brain was examined post-mortem and a lesion from vascular abnormality was found; the lesion involved both cingulate gyri and the corpus callosum. From analysis of this and other cases Nielsen concludes that the

patterns of conation (desire to move) which are impaired in such cases have a mesencephalic component in the region of the aqueduct of Sylvius, a diencephalic component involving mammillary bodies and anterior thalamic nuclei, and a cortical component involving the cingulate gyri.

The role of the frontal lobes in affective or emotional behavior has been discussed by a number of writers. Fulton (33) has summarized recent clinical and experimental studies of frontal lobe function. In the historical introduction of his monograph, he lays particular stress on the contributions which came from the development of experimental psychobiological methods.

Landis (53) has de-emphasized the intellectual and has stressed the

affective changes that take place in patients after psychosurgery involving the frontal lobes. He believes that patients who benefit from frontal lobe surgery are those suffering from a kind of mental state for which the term "anguish" is suggested. Landis, further, suggests that

this anguish is a specific experience which occurs when some agent carried in the blood stream sensitizes cortical tissue, usually tissue in the prefrontal areas in persons who are susceptible. This susceptibility is activated largely by the influx of intra-organic sensory impulses probably projected from the thalamic nuclei.

The patterns of autonomic responses during stress or emotion have been the subject of two studies. Ax (10) found significant differences in the patterns occurring during fear and anger. He relates these differences to the differences in physiological reactions to epinephrine and norepinephrine. According to results obtained by Lacey (52), individuals not only tend to exhibit characteristic patterns of autonomic response to stress, but they also "tend to respond with maximal activation of the same physiological function in a variety of stress situations."

LEARNING, MEMORY, AND PROBLEM SOLVING

Experimental ablation and brain damage.—As a test of "equipotentiality" of cortical function, Pickett (76) studied the retention and relearning by blinded rats of a maze habit controlled only by somesthetic cues. Lesions up to 41.6 per cent in the posterior areas of the cortex did not affect the learned habit. In an enclosed maze lesions of sensorimotor areas produced an impairment in postoperative performance, the amount of impairment increasing with size of lesions.

The results of a study by Peterson & McGiboney (75) indicate that it is extremely difficult to re-establish the use of the preferred hand in rats in which transfer of preference has occurred after brain damage. Age of adult animals, delay in starting retraining, and amount of retraining were not important factors in effectiveness of retraining. Shurrager & Dykman (89) have described the walking behavior of kittens and of a puppy in which the spinal cords were completely transected at the thoracic or lumbar level. Controlled stimulation after operation was an important factor in the development of walking patterns.

In a repetition of earlier curare experiments, but using *d*-tubocurarine which has no marked cortical effects, Lauer (55) found that, after conditioning in a state of complete curare paralysis, conditioned motor responses occurred without further training upon recovery from the paralysis.

A group of normal monkeys and a group with temporal neocortex and allocortex ablations were trained on three visual discrimination problems by Riopelle & Ades (78). Operated animals were retarded in their learning of the visual tasks, the degree of retardation increasing with increase in difficulty of the task.

A conditioned avoidance response to sound was found by Stewart &

Ades (93) to be retained after two-stage bilateral ablation of the superior temporal gyrus if a period of seven or more days was allowed to elapse between operations. Prefrontal lobectomy before the two-stage temporal lobe ablation led to loss of habit after the final operation even with a long time interval between temporal lobe ablations. From this and earlier experiments the authors conclude that some kind of "spontaneous reorganization" can occur in the central nervous system so that a learned auditory habit is retained without retraining trials when the superior temporal gyri are ablated in two stages; but that this reorganization appears to take place only when there is a period of some seven days between ablations of the two temporal lobe regions and when the corpus callosum, anterior commissure, and prefrontal lobes are intact.

Riopelle *et al.* (79) compared the learning of visual discriminations by four groups of monkeys: (a) animals with prestriate areas ablated bilaterally (Visual Group); (b) animals with extensive unilateral decortication plus frontal lobe ablation on the contralateral side (Frontal Group); (c) animals with extensive unilateral decortication plus ablation of a region between the intraparietal and lunate sulci on the contralateral side (Posterior Group); (d) unoperated animals with similar experience in visual learning tasks to those of the above three groups (Normal Group). On a simple discrimination of dissimilar objects the performances of all operated groups were about the same as that of the Normal Group. In pattern discrimination and mirror-image pattern discrimination the Posterior Group was greatly inferior to the others. Both Frontal and Posterior Groups were inferior on serial discrimination tests. It should be noted that all animals in these groups had received a good deal of training preoperatively on a variety of visual discrimination problems and it is implied, although not specifically stated in the report, that there was a long postoperative recovery period before the start of the experiment here described.

Meyer, Harlow & Ades (62) found little impairment in the performance of monkeys on delayed response and oddity problems after bilateral ablation of prestriate areas. They compared these results with those of earlier experiments by Ades (1), Ades & Raab (2), and Lashley (54) and offer the hypothesis that the differences in findings are due to the amount of training on a given class of problems used in a particular experiment; "habits applicable to the solution of a particular problem are lost" but "acquisitions applicable to the solution of a class of problems are spared after preoccipital ablation."

Harlow, Meyer & Settlege (39) have shown that the oddity problem, a problem in which three objects, two identical and one different, are presented to the subject and the selection of the dissimilar object rewarded, is a sensitive indicator of large unilateral cortical ablations. Contralateral ablation of either frontal or posterior areas adds to the deficit. In contrast to other types of learning problems which have been studied, the impairment of ability as brought out by the oddity problem continues even after training on many successive problems of the same sort.

Warren & Harlow (102) have compared the learning of visual discriminations by three groups of monkeys: a normal group, a group with lesions of the frontal lobes, and a group with lesions of parieto-temporo-occipital areas. A recovery period of 14 to 18 months was allowed for the operated animals; during this period all animals had experience on a long series of laboratory tests. It was found that the Frontal and Posterior Groups were still inferior to the Normal Group on problems involving discrimination of color-pattern, of form-pattern, and of size-pattern. All groups were about equal on object discrimination problems.

Confirming numerous earlier studies, Blum, Chow & Blum (18) found that impairment of ability in a delayed response problem follows ablation of frontal granular cortex. Contrary to findings reported by Wade (101) and Pribram (77), they did not find that sedative or excitant drugs (pentobarbital [Nembutal; 5-ethyl-5-(1-methylbutyl) barbiturate] and amphetamine [Benzedrine; α -methylphenethylamine sulfate]) help to overcome the impairment. They attribute the difference in results to the length of time elapsing between operation and delayed response tests; drugs may hasten adjustment to the delayed response test in newly operated animals. The animals tested by Blum, Chow & Blum had a postoperative recovery period of six months to two years.

In further analysis of frontal lobe function, Blum (19) tested monkeys with frontal cortex ablations on an auditory discrimination problem and on delayed responses to auditory and visual cues. Lesions were made in the dorsal, midlateral, or ventrolateral parts of the frontal granular cortex; in some cases all parts were ablated. Animals with midlateral lesion showed quite severe deficits in performance, those with dorsal and ventrolateral lesions, only slight deficits. The impairment was not specific to a given sense modality. Blum suggests an explanation in terms of a general perceptual loss or reduced "scope of responsiveness."

Evarts (30) has found that ablation of prestriate cortex (area 18) in the monkey has little or no effect on retention or learning of a conditional problem in which the animal selects a red stimulus in the absence of sound and a green stimulus when sound is present. Although some small parts of area 18 were left intact in all animals, Evarts does not think that they were large enough to account for mediation of the visual habits via cortico-cortical pathways but instead suggests as possible alternative mechanisms, cortico-thalamo-cortical circuits or the diffuse thalamo-cortical projection system.

To test the hypothesis that lateral granular and transitional areas of the frontal cortex are important in certain problem solving situations and that the medial granular and agranular areas are concerned with emotional expression, Rosvold, Pribram & Mishkin (83) tested three groups (nine animals) of baboons on a delayed response problem and rated them over a period of time for emotional responses. One group was kept as a control group, the other two underwent ablations appropriate to the hypothesis in question. The animals with lateral-frontal ablations were found to suffer

deficit in the delayed response tests; the medial-frontal group showed little or no deficit. No persistent changes in emotional behavior were observed.

Studies of intellectual functioning as measured by complex sorting tests, tests for hidden figures, and reasoning tests have shown that war veterans with penetrating wounds of occipital or parieto-occipital regions suffer as much impairment as those with damage to frontal areas. Both groups do more poorly than control subjects with peripheral nerve injuries according to reports by Battersby, Teuber and Bender (13) and Teuber, Battersby and Bender (97).

Levine (57) has compared the scores made on six subtests of the Wechsler-Bellevue by three groups of blinded veterans: (a) those blinded as result of injury to occipital cortex; (b) those blinded by direct damage to eyes; and (c) those blinded by direct damage to eyes but having also brain injuries in areas other than occipital cortex. The occipitally blinded subjects showed the greatest impairment; the subtests most affected were the Arithmetic and Digit Span tests. The group with peripheral plus central damage was second in degree of impairment. Levine concludes that the results, although they cannot be accurately assessed because of ignorance of exact brain lesions, tend to support Lashley's hypothesis that the visual cortex is involved in intellectual functions of a nonvisual nature.

An immediate postoperative decrement in performance on psychomotor tests involving attention was found by Sheer (88) for schizophrenic patients with frontal lobe operations. Pentobarbital sedation improved performance and, in most cases, test performance was back to preoperative level in 6 to 12 months after operation.

After complete or nearly complete transection of the corpus callosum in human subjects, Smith (90) found no effect upon learning or upon bilateral transfer of learning in a mirror drawing task. In learning and in bilateral transfer of learning of a stylus maze, results were not consistent. On the basis of these results and the results of a number of animal studies, Smith suggests that

there seems to be no pattern of neurones within the cortical layers or between the two cerebral cortices, the interruption of which consistently disturbs either learning or the dynamic functions of transfer and generalization related to such learning.

Changes in emotional behavior, in performance on psychological tests, and in the EEG have been described by Apter *et al.* (6) for a patient with pheochromocytoma (tumor of adrenal medulla). General impairment of function is attributed to cerebral cortical dysfunction resulting from disturbance of cerebral circulation. This impairment is reversible as evidenced by the marked improvement shown by the patient after removal of the pheochromocytoma. Apter, Halstead & Heimburger (7) have also described changes in personality and in performance on a standard battery of psychological tests of a group of patients with essential hypertension. Impairment on the psychological tests was similar to that shown by patients with brain damage.

Drugs and chemicals.—In an attempt to discover if peripheral autonomic responses play a role in the drive of fear, Auld (9) trained and extinguished groups of rats in an escape conditioning situation. One group of rats was given intraperitoneal injections of TEA² chloride as a means of partially blocking transmission of autonomic impulses through ganglia; a second group of control animals received injections of saline. Animals given TEA during training were slower than their controls on both training and subsequent extinction; those given the drug during extinction were slower than the controls during extinction. Assuming that TEA has no detrimental effect other than partial blocking of autonomic ganglia, Auld concludes that the results favor the interpretation that the fear drive is partially dependent upon peripheral autonomic responses.

A number of experiments have dealt with the detrimental effects of anoxia and particularly anoxia at birth. Hurder (45) found impairment of maze performance in adult rats subjected to anoxic anoxia; Armitage (8) found impairment on learning and reasoning tests in rats as the result of administration of barbiturates to pregnant females prior to parturition; and Becker & Donnell (15) found marked deficits in the maze performance of guinea pigs subjected to asphyxia in utero by clamping the maternal uterine vessels. Nielsen & Courville (73) from studies of human patients believe that birth injury and asphyxia may be causes of small focal areas of cortical damage which act as "trigger mechanisms" in epileptic convulsions.

Studies of the effects of glutamic acid on learning and intellectual abilities are like studies of latent learning. One investigator reports positive results, another negative; and, since the variables manipulated and measured are never quite the same, comparisons among the studies are almost impossible. Zabarenko, Pilgrim & Patton (107) added 3 per cent L(+)-glutamic acid to the diet of rats and compared their performance on learning situations with that of animals fed a standard diet. No differences were noted. Porter, Griffin & Stone (76a) found that rats fed diets designed to affect glutamic acid metabolism did not differ in performance on a water-maze from animals fed on a standard diet. In contrast to these results, Sweet (94) has reported that the offspring of mothers given supplementary glutamic acid during gestation perform significantly better on a double alternation elevated maze than the offspring of mothers fed a standard diet.

Glutamic acid treatment of mental defectives has been reported in two recent studies. Milliken & Standen (67) found no evidence that administration of glutamic acid (in the form of sodium glutamate) had any effect on cognitive ability as measured by such tests as Stanford Binet, Wechsler-Bellevue, Kohs' blocks, and Healy's picture completion. Furthermore, there was no evidence of changes in performance on personality tests. In two cases, however, some improvement of emotional behavior was noted during periods of glutamic acid feeding. Albert, Hoch & Waelsch (4) fed glutamic acid in the form of L-glutamic acid to mental defectives of similar classification to those used in the Milliken & Standen study. A significant improvement in performance on the Stanford Binet was found during the period of glutamic

acid feeding. The authors point out that the amount of improvement, though statistically significant, has no clinical significance. They also suggest that the improvement in performance on mental tests may be in part or entirely due to improvement in ability to use existing intellect through the elimination of disturbing emotional factors. A possible explanation of failure of agreement among different investigations is proposed: there may be a difference in the action of L-glutamic acid and of sodium glutamate.

BIOCHEMICAL AND NEUROANATOMICAL CHANGES IN MENTAL DISEASE

In a variety of cases with neurological and psychotic disorders, no changes in phospholipid and cholesterol concentrations were found by Fisk, Chanutin & Klingman (31). Slight evidence of increase in sphingomyelin concentration was noted in the patients with neurological diseases. Ritchie (80) failed to find evidence for adrenergic or cholinergic substances in the peripheral venous blood of psychiatric patients. Altschule, Siegel & Henneman (5) found that the blood glutathione level of untreated manic depressive, involuntal, and schizophrenic psychotics and of some severe neurotics was in or below the normal range. After shock treatment, there was a rise in the glutathione level, the increase being most noticeable in the cases having the lowest levels prior to treatment.

Abnormal changes in the cytochemistry of nerve cells of biopsies obtained at the time of prefrontal lobotomy or immediately after death in cases of dementia praecox have been described by Papez & Bateman (74). A most interesting finding is the presence in the nerve cells of inclusion bodies which increase in size and number with severity of the disease. The staining characteristics of the inclusion bodies "suggest that they contain phospholipids and ribonucleic acid. Their destruction of cytoplasm suggests the presence of some proteolytic activity." The resemblance of the inclusion bodies to certain of the animal viruses is pointed out.

Himwich (41) has discussed the relation of defects in biochemical processes of the brain, particularly those having to do with energy transformation, to cerebral function. Abnormalities which occur as the result of malfunction at different levels of energy transformation are described. For example, at the first level (the oxidation of glucose), malfunction is evidenced by the clinical changes commonly noted in hypoglycemia, anoxia, or avitaminosis. At a second level, a defect in storage of energy in the energy rich phosphate bonds may be a factor in the production of narcosis, and, at a still higher level, the blocking of acetylcholine synthesis may result in delirium. The psychotic reactions produced by adrenocorticotropin, cortisone, and lysergic acid diethyl amide tartrate are also discussed.

BOOKS AND MONOGRAPHS

Limitations of space do not permit a review of a number of excellent books and monographs which have appeared during the past year. For the sake of completeness they are here listed: *Cerebral Mechanisms in Behavior*,

The Hixon Symposium (49), *Doubt and Certainty in Science: A Biologist's Reflections on the Brain* (104), *Symposium on Brain and Mind* (95), *Biology of Mental Health and Disease* (16), *Frontal Lobotomy and Affective Behavior* (33), *Visceral Innervation and Its Relation to Personality* (51), and *Emotions and Clinical Medicine* (26).

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ABNORMALITIES OF BEHAVIOR¹

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The study of behavior disorders appeals to a wide variety of scientific tastes. The range of relevant topics extends all the way from infantile psychodynamics to brain metabolism, both of which, as it happens, received considerable research attention during the past year. Even the most general treatises rarely undertake to patrol so far-flung a frontier. The revised and expanded textbook, *Principles of Abnormal Psychology*, by Maslow & Mittelmann (45), takes the dynamics of psychological illness as its central theme. *Dynamic Psychiatry*, edited by Alexander & Ross (2), is essentially a textbook from the point of view of psychoanalysis. On the other hand, Alvarez (4), contributing to the practitioner's library an ample survey of the diagnosis and management of functional disorders, sets his account squarely in the framework of biology and general medicine.

Alexander & Ross conceive of their book as part of an important historical trend in psychiatry. Liking Freud to Darwin in the sense that both men advanced a descriptive science to an explanatory stage, they point out that psychoanalysis, at least in the United States, is fast losing its position as an isolated specialty and is "becoming recognized as a basic science both in psychiatry and in the social sciences." The workers at the Chicago Institute have indeed played an active part in speeding the assimilation of psychoanalysis by psychiatry, and it is appropriate that they should take the lead in surveying current accomplishments. Of the 15 contributors to *Dynamic Psychiatry*, however, only seven are members of the Institute staff.

THE LEARNING OF BEHAVIOR DISORDERS

It has long been implicit that psychodynamic concepts would some day be translated into the more basic categories of motivation and learning. To the extent that behavior disorders are not determined by abnormal conditions in the brain, they must be conceived as consequences of learning. In 1950, Dollard & Miller (13) took a major forward step toward this translation when they undertook to apply reinforcement theory systematically to such problems as the learning of neurotic defenses, the acquiring of neurotic symptoms, and the changes that take place during psychotherapy. Within the past year there have been several further contributions to this general problem.

Learning is the central theme of *Behavior Pathology* by Cameron & Magaret (10). Less explicit than Dollard & Miller with respect to its basic learning theory, it is considerably more searching as regards the varieties of

¹ The survey of the literature to which this review pertains was completed in June, 1952.

pathological behavior that need to be interpreted as consequences of learning. The authors start from the position that pathological reactions "always represent the exaggeration, distortion, or impoverishment of everyday normal behavior"; such reactions consist of "extravagant, restricted or inappropriate use of techniques which everybody uses in attempting to reduce the tensions of need and anxiety." On this basis, they attempt to show that all the major forms of behavior disorder result from patterns of reaction that have become overgeneralized, over-learned, and self-perpetuating, so that they do not yield when circumstances render them inappropriate. Anxiety, depressions, manic excitement, delusional states, and schizophrenic "de-socialization" are included among the disorders that can be interpreted as end products of cumulative learning. The authors consider traumatic events to be relatively unimportant, and they do not insist upon the crucial significance of the first five years of life. Social learning, they say, "much of it casual, incidental and unrecognized," goes on continuously throughout life and culminates in behavior disorder only under certain more or less definable contemporary conditions. This book should have an important effect on the field of abnormal psychology, and it deserves painstaking comparison with the more psychoanalytically oriented theories of Dollard & Miller.

The learning of behavior disorders is treated in a somewhat different fashion by Ausubel (5) in *Ego Development and the Personality Disorders*. Ausubel's intention is "to present a systematic and comprehensive developmental theory of psychopathology that is consonant with modern concepts of genetic psychology." In pursuing this goal he "seeks to synthesize the fields of child development and abnormal psychology around the central theme of ego development." Positing a stage of ego omnipotence in earliest childhood, he further specifies his central theme as ego "devaluation" and "maturation," and he uses materials from child study, including the shrewd observations of Gesell (23), to fill out the details of this growth. Most behavior disorders reveal some degree of failure in the process of ego devaluation; social relationships are continually injured by traces of omnipotence and excessive claims. Such failure is apt to occur when poor conditions in the family circle make it hard for the child to "satellize" as a family member. When "satellization" is successful, however, there remains another possible danger, failure to become "desatellized" and emancipated as a mature and independent person. This is the basic difficulty in the inadequate, pleasure-seeking psychopath (as contrasted with the aggressive, asocial psychopath) and in the "evolutionary schizophrenic" (hebephrenic and simplex varieties) as contrasted with the "reactive schizophrenic" (paranoid and catatonic). These examples show a contemporary trend in abnormal psychology that is by no means confined to Ausubel's work: that of rejecting the older psychiatric categories when developmental analysis provides a more valid basis for classification.

A third book on the learning of behavior disorders comes from Silverberg (66) under the title *Childhood Experience and Personal Destiny*. Silverberg

is one of that growing company of psychoanalysts who feel the necessity to make important revisions in Freud's conceptual scheme. His changes are made partly in the directions favored by Sullivan (70), stressing the concept of self-esteem, but Silverberg rejects Sullivan's exclusive emphasis on the social shaping of the self and introduces his own concept of "effective aggression" to account for "the ego's efforts in the direction of achievement as well as of defense." He observes:

Throughout life self-esteem has these two sources: an inner source, the degree of effectiveness of one's own aggression; and an external source, the opinions of others about oneself.

It is characteristic of neurotic patients that they lack the inner source and are obliged to depend upon the unsteady outer source. Silverberg's detailed views on the learning of behavior disorders are derived from a painstaking reanalysis of what is learned during the first five years as the child copes with problems of "orality and deprivation," "discipline," and "rivalry and genitality." In sharp contrast to Cameron & Magaret, he affirms that

mental illness originates in the adaptations made to traumatic experiences in early life . . . in its details the illness bears the impress of those experiences and the child's reactions to them.

All three of these attempts to conceptualize the learning of behavior disorders place a great deal of emphasis on the ego, the self, self-reactions, and self-esteem. These important concepts were also discussed by others during the course of the year. In a scholarly paper, McKinney (48) compares ideas of the self derived from the observation of schizophrenic patients with the older ideas coming from social psychologists and sociologists. Redl (57), in a discussion of techniques of ego support, describes four varieties of ego disturbance and expresses the belief that it is possible to distinguish at least 30 such patterns. As a contributor to a symposium on the healthy personality, Erikson (16) carries forward and clarifies his concept of ego identity, developed previously in his book *Childhood and Society* (15). "By identity," he says, "I mean a sense of being identical with one's self and also of being identical with a workable role provided for one in one's culture." In all of these papers it is clear that the concept of the ego has felt the growing impact of the social sciences.

That the theory of behavior disorders is well on its way toward conceptual ripening is apparent in the first volume of a projected five-volume series, *The Integration of Behavior*, by French (17). The purpose of this work is to build a picture of the integrative processes that underlie all behavior. These may be presumed to operate most effectively in normal and successful behavior, but they can be examined also, though somewhat fragmented, in dreams and neurotic behavior. The present volume, which has its own subtitle, *Basic Postulates*, works to establish general propositions, but there is concurrent scrutiny of the productions of a patient during two years of psychoanalytic treatment. French is entirely free from the professional bias of

the physician who notices lawfulness only in disease. Into one of his systematic propositions, for instance, he introduces "the sustaining influence of hope" on effective, goal-directed action; and he places emphasis on the cognitive processes and on planning in the integration of behavior. All the contributions reviewed in this section can thus be said to bring the day nearer when the theory of behavior disorders and the theory of natural development will utilize a common conceptual scheme.

EARLY CHILDHOOD ORIGINS

When we turn from theory to clinical investigations we find that an unusual amount of attention has been devoted to the first year of life. The neuroses of childhood are fairly well understood; what is of chief interest now is to work out the origins of more baffling, less treatable disorders such as childhood autism and schizophrenia, psychopathic behavior, and pseudo-feble-mindedness.

Reports are now available on the second and third round tables conducted by Karpman (36, 37) on psychopathic behavior in infants and children. The participants in these conferences agree that the diagnosis of psychopathic behavior must be used sparingly. Impulsive and antisocial behavior in childhood is to a certain extent universal and to a certain extent symptomatic of essentially neurotic conflict. It should be called psychopathic only in those rather rare cases in which the young patient seems to have been from the start unable to establish object relationships, hence unable later to experience guilt and to internalize a code of restraints. One cannot say with certainty that organic complications, possibly subtle brain damage, play no part in these cases, but there is at least a strong impression that the failure of object relationship results from early maternal handling that is aloof, cold, mechanical, or frankly rejecting. The initial basis for object relationship is never laid, and this vital lack in the child's development makes him become not only a behavior problem but an extremely poor subject for psychotherapy. An independent report on a notorious homicidal criminal by Smykal & Thorne (67), emphasizing a consistent childhood history of rejection, underprivilege, trauma, and lack of emotional attachments, can be taken to support these general ideas. Some members of Karpman's symposium, however, seemed to doubt that the concept of psychopathic personality was ever applicable to children. Perhaps they would even agree with Miller (50) that an adequate psychodynamic understanding of adult psychopaths always makes it possible to classify these cases in the categories of neurosis or psychosis.

Several studies pursue the elusive theme of the origins of schizophrenia in childhood. For the most part, these origins are inferred from the productions of adult patients during treatment, but a few reports deal directly with children who exhibit schizophrenic behavior in the early years. Kanner (35), who prefers to call these conditions "early infantile autism," discusses an important feature of the behavior, namely, the child's desire to live in a

static world where no change is tolerated, and his insistence that adults keep this world unchanged. Evidence that cold, mechanical maternal behavior plays a part in setting up this disorder is contained in a case report by Darr & Worden (11), a paper that is of unusual interest because it deals with a schizophrenic episode in a patient of 32 who had been seen 28 years before at the Phipps Clinic and given a diagnosis that obviously corresponds to Kanner's "early infantile autism." The most striking piece of evidence, however, comes from Despert (12), who studied the mother of an autistic child while the mother was pregnant with a second child and during the early stages of the second child's upbringing. The mother's obvious lack of empathy and of real desire to have children caused Despert to recommend a motherly nurse for the second child. This was successful, but when the nurse could not be retained, and the mother herself took full charge, the child began almost at once to develop in an autistic direction. Despert's case would appear to be one of those "natural experiments" that greatly strengthens an hypothesis otherwise based largely upon inference.

Relevant at this point is a report by Sperling (68), even though it deals with neurotic rather than autistic children. On the basis of concomitant psychoanalyses of mothers and their children, Sperling concludes that the neurotic features of the child's behavior can often be understood as unwitting, hypnotic-like responses to the mother's unconscious wishes. One little girl, for example, was virtually phobic about being separated from her mother; it turned out that the mother was unconsciously all but phobic about having the child out of her sight. These studies shed much light on the close interaction between mother and child.

The treatment of adult schizophrenics brought forth during the year several speculative contributions as regards childhood origins. There is, in the first place, Sechehaye's book, *Symbolic Realization* (65), based on years of therapy, ultimately successful, with a schizophrenic girl. Sechehaye believes that the treatment turned in the right direction only when she herself was able to respond to the patient's unfulfilled infantile needs and to do so in the symbolic ways in which the patient was currently feeling them. Thus the patient's refusal of most foods finally yielded when the therapist offered each food as a present, thus symbolically gratifying the frustrated needs of the nursing period. Sechehaye assumes that the frustrated needs persisted but became so laden with guilt that they could never be translated into undisguised verbal terms. Therefore the guilt can be relieved and the symbolic and delusional gratifications abandoned only when the needs are for a while met in their primitive preverbal forms. There is considerable similarity between her method and the one developed by Rosen (59).

Two contributions by Szalita-Pemow (71, 72) support the view that the first two years of life, if not even the first few months, are crucial in the development of schizophrenia. The argument is based largely on the lack of organization in the productions of the adult schizophrenic, the tendency of his thoughts to exist like separate islands which cannot be integrated into

effective action. This fragmented quality of mental life is attributed to a lack of regularity and consecutiveness in the building up of early experience, this in turn being due to emotionally unreliable behavior on the part of the parents. If the mother is anxious about certain topics, for example, the infant may feel this anxiety and fail to integrate these topics with the rest of his experience. Imaginative capacity is not injured, but organization is early rendered ineffective. Somewhat similar reasoning is used by Eickhoff (14), who speaks of the building up of the body image and other important constructs from consistent sequences of sensory experience. Failure to organize these basic categories of experience might result from lack of the stimulation that comes from steady mothering, but Eickhoff points out that defects in the neurological systems concerned with general sensation might have a similar and even more drastic effect. The case reported in her paper, having various evidences of neurological disorder, lends itself to the second interpretation.

Less committed to the earliest months is Powdermaker (55), whose speculations are founded on work with mild and ambulatory cases of schizophrenia. Powdermaker believes that the confused character of schizophrenic acts and utterances results from a specific kind of failure in developing a feeling of reality. The patients simply have no confidence in the validity of their own percepts, thoughts, and feelings, at least for purposes of communication, and they lack this confidence because their parents, through anxieties of their own, have rejected the ideas they put forth. If the child makes an aggressive remark to someone with whom the mother is talking, for example, and the mother anxiously discounts the remark, the child is left in confusion over the validity of his own experience. If he solves this kind of problem by unwillingly accepting as real what the mother calls real, he will form a split between what he experiences as real and what the world calls real. The crucial period for this kind of mistraining is the time when self and not-self are becoming clearly differentiated, and the specific source of pathology is the parents' anxiety, which makes it hard for them to accept and confirm their child's experiences.

The search for childhood origins was also pressed in connection with intellectual disabilities. The twenty-fifth anniversary of the Wayne County Training School in Michigan was the occasion of a symposium by McCandless and others (47). In this symposium, Bartemeier comments on children who, though placed in a training school, are really not defective in any structural sense but have been emotionally starved in early infancy. Their apparent mental retardation reflects "a mismanagement of their instinctual needs during the early stages of their development," and they thus show "how intimately the learning process depends upon a continuing maternal affection which provides a continuing understanding of basic biological needs." McCandless also emphasizes the emotional surroundings of learning, though at later stages of development, pointing out that bright children have ample opportunity to learn not only interesting contents but also attitudes

of confidence, "relatively constructive expectancies," whereas the dull have fewer learning experiences of a constructive intellectual sort and in addition acquire more " 'self-defeating' behaviors." A step toward refining these general ideas is made by Sperry, Staver & Mann (69), who try to trace, in a small group of intensively studied children, the specific events, attitudes, and fantasies that caused the child initially to inhibit certain aspects of the learning process. They show that destructive fantasies linked with severe anxiety probably account for several of these learning inhibitions.

THE PROBLEM OF RESEARCH ON PSYCHODYNAMICS

Although these several contributions to childhood origins seem to be somewhat at variance, they can all be said to work on a common task. Whatever their particular emphases, they all call attention to some aspect of early childhood development and offer suggestions as to how it can go astray. In some way behavior and experience are deflected from their normal course of growth: early needs are left unsatisfied, maternal stimulation and affection are not provided, anxieties and guilts are allowed to arise prematurely, experience is fragmented by emotionally unreliable parental behavior, the child's self-feeling is prevented from investing his ideas and perceptions of reality. We do not know enough about the child's early development to decide upon the merits of these different proposals. We cannot even assume that some are right and some wrong. All might be wrong; or, more likely, all might call attention to significant aspects of growth without conclusively demonstrating their central importance.

Two general points of criticism are worth noticing. Erikson (16) points out a tendency to seize upon maternal behavior as the cause of all trouble. He gives his impression that a certain unresponsiveness or other peculiarity in the child contributes to disordered development and has an unfavorable effect on the mother's behavior. Karpman (36) criticizes a lack of sharpness in most reasoning about psychodynamic processes, noting that we have recently attributed a good many different results—autism, schizophrenia, psychopathic behavior, intellectual retardation—to the single cause of cold and mechanical maternal behavior. It is unfortunate, incidentally, that if errors are being made they should all be in the direction of blaming the mothers. Witmer (73), in a symposium on normal development, reports her finding that most mothers throughout the country who are trying to bring up their children in the light of current psychological knowledge have been badly intimidated by the two ideas that the child's fate is fixed at an early age and that the mother is solely responsible for that fate.

How can we make our knowledge of early childhood psychodynamics more certain and more discriminating? A traditional answer would be that the time has come for the naturalist, in this case the clinician, to give place to the systematic experimentalist. Publications during the year show that experimentalists have not wholly overlooked the possibility of making a contribution. From the Fels Research Institute comes a report by Lasko

(39) describing recent developments in the observation and rating of parental behavior, a basic step for the large-scale study of parent-child relationships. Lasko points out that maternal behavior changes "as the mother becomes more experienced with children and more acclimated, perhaps, to her role as mother." This fact of change greatly complicates the problem of quantitative analysis. In two papers, Henry & Warson (27) and Henry (26) develop the idea that family configurations and their effects on development can be described with system and precision. They consider that the rigid behavior characteristic of neurotics is in fact a rigid interactional pattern which is to some extent shared by others in the family. Such patterns can be transmitted, through learning, in family lines. Nowlis (52) describes the problems encountered at the Iowa Child Welfare Research Station in trying to work out and define significant variables for the quantitative study of parent-child relationships. Scales were developed, largely by Whiting, to measure maternal punishment, maternal nurturance, maternal responsiveness to the child's aggression, maternal encouragement and instruction, and other features of behavior, the source of information being extensive recorded interviews with the mothers of nursery school children. Finally, Murdoch & Whiting (51) jointly report upon the use of these scales in cross-cultural studies, showing significant differences in child training pressures as between, for example, extended and nonextended families.

Valuable as these studies may be in their own right, they do not help us as yet in arriving at clearer ideas about the learning of behavior disorders in childhood. And this is not wholly because the studies are in early stages, searching for variables, dealing with rating scales, trying to fashion the very tools without which quantitative investigation cannot even start. There are two very serious inherent difficulties in using quantitative studies to disclose the type of psychodynamic process—ultimately, learning process—that is generally thought to be crucial in producing abnormalities of behavior.

In the first place, the sacrifices that must be made for quantification tend to obscure, very likely fatally, the significant individualized patterns of events that may be presumed to be the real determiners of development. Ratings can perhaps give us valid information about the general atmosphere of the home and of the relationships between parent and child, but atmosphere is an effective influence only in so far as it is embodied in particular transactions, and the consequences of these transactions are by no means a simple function of the atmosphere. As Erikson (51) pointed out in a comment on Murdock & Whiting's paper, the "severity" of a frustration cannot be rated without reference to more than itself: "culturally meaningful sacrifices are not the same as meaningless frustrations." Furthermore, the effect of certain transactions which unexpectedly deviate from the atmosphere to which the child is accustomed may be of quite dramatic importance in shaping further development. If witnessing the "primal scene," for example, is important, it is because it blasts the child's expectations as to how his parents ought to behave. Thus, even if we know that there is a significant

relation between, let us say, rated severity of frustration in childhood and the frequency of occurrence of later behavior disorder, we cannot use this information to illuminate actual psychodynamic processes.

In the second place, there is a serious inherent difficulty in correctly observing the events that clinicians have postulated to be of vital importance in early childhood development. The scientist suffers from the handicap that he can never be there when the events take place; he cannot systematically witness social learning, "much of it casual, incidental and unrecognized," as Cameron & Magaret remark, nor can he watch the crucial episodes in which cold, mechanical, maternal handling produces one of its postulated effects on the infant's inner development. These things can rarely be observed even in a single instance. To study them through ratings based on interviews with parents is to commit oneself in advance not to precision but to a fatal built-in lack of precision.

The trend of the year's studies on early childhood origins thus brings us to a chastening conclusion. This is a realm in which we are probably never going to know exactly how everything works. To the extent that we can know more, however, our knowledge is most likely to come from better and more disciplined clinical observation. Occasional "natural experiments," like the one described by Despert (12), can be helpful, but the real hope of progress lies in the improvement of the clinical worker as a scientific observer. When the clinician becomes fully aware of the difficulties of his task, when he realizes the pitfalls of premature generalization and makes a regular practice, like Darwin, of writing down contradictory observations in his notebook, then we shall get such knowledge of childhood psychodynamics as the nature of the subject matter permits.

BIOCHEMICAL INVESTIGATIONS OF SCHIZOPHRENIA

The vast proportions of the research attack on schizophrenia come out in a 10-year general review by Bleuler & Benedetti (9), which makes reference to no less than 1101 titles. Although the attack is being pressed on many fronts, activity seems to be momentarily highest in the biochemical sector. Recent attempts to relate schizophrenia to hormonal peculiarities are concisely surveyed in a paper by Malamud (42). The conclusions reached by Hoagland (30) and his associates with respect to the adrenal cortex are also the subject of a relatively condensed review.

Hoagland finds that the activity of the adrenal cortex in schizophrenics is normal in resting states but deficient in response to stress. The adrenal cortex is not activated through neural channels but rather by ACTH,² and the deficiency in the patients is interpreted as a refractoriness of the cortex to stimulation by ACTH, which itself appears to be produced in sufficient supply. This refractoriness is greater in some patients than in others; those in whom it is less great are likely to respond more favorably to electro-

² ACTH (adrenocorticotropin).

shock therapy. Causal relationships cannot as yet be fully clarified. Chronic overactivity in the hypothalamus, leading to excessive chronic release of ACTH by the pituitary, might conceivably produce an acquired refractoriness in the adrenal cortex. This hypothesis could readily be connected with psychological theories which emphasize the role of anxiety and other emotions in the genesis of schizophrenia. On the other hand, there is the possibility of a genetic deficiency which would probably have to be conceived as affecting all responses to stress in life. Perhaps this hypothesis could be related to the alleged unresponsiveness of the infant who later becomes autistic or schizophrenic. If a genetic deficiency is involved, Hoagland suggests that it might be traced to an enzyme deficiency, and he calls attention to various researches which establish linkages between enzymes and genes.

Taking a somewhat different line, the Sacklers and others of the Creedmore Group propose that schizophrenia is caused by chronic excessive activity of the adrenal cortex. In three papers (62, 63, 64) they report upon therapeutic experiments with thyroid, gonadal steroids, insulin, and histamine, all of which may be regarded as physiological antagonists to the adrenocortical substances. It is assumed that the adrenal overbalance is periodically compensated by the natural production of antagonists but that this compensation cannot always be maintained. The Sacklers note that on the basis of this reasoning they predicted, some years ago, that ACTH and cortisone, when heavily administered for the treatment of some other kind of disorder, would tend to produce temporary psychoses of schizophrenic form—a prediction that has since become an observed fact.

The production of temporary psychotic states by means of drugs has attracted considerable notice. Mayer-Gross (46) surveys the history of this type of investigation since the time of Kraepelin, and reports especially on the effects of mescaline and lysergic acid diethylamide. Of particular interest is a paper by Osmond & Smythies (53) noting the similarities between mescaline intoxication and acute schizophrenic episodes and calling attention to the close biochemical similarity between mescaline and epinephrine. Such studies suggest that the type of experiment favored by Kraepelin may now be ready to fit significantly into other methods of approach to the biochemical aspect of mental disorder.

There is an intriguing simplicity about the theories of schizophrenia that emerge from biochemical studies, even though the studies themselves require vast technical complexities. It is easy to forget that the experimental sophistication needed to measure the output of the adrenal cortex—a full-scale research problem in its own right—does not guarantee that the resulting theory of schizophrenia will be in the least sophisticated. Perhaps some theory about the adrenal cortex will seem well suited to explain the unresponsiveness and affective flatness shown by schizophrenic patients, but how sure can we be that patients regularly exhibit these traits? It is important that investigators should not assume that we currently possess final valid descriptions of the way in which patients behave. An investigation

from McGill University by Malmö, Shagass & Smith (43) provides a much-needed corrective to the oversimplifications that often appear in biochemical studies.

These investigators required a variety of test performances from chronic schizophrenic subjects, at the same time taking certain physiological measurements. On an over-all measure of responsiveness, the patients did not differ from controls, but there were significant and meaningful differences when different types of performance were considered separately. It was found that the patients were more reactive than controls with respect to heart rate, blood pressure, and muscular tension, less reactive in tasks involving overt action such as mirror drawing, size discrimination, and pressing a button to signalize the experience of pain. In other words the schizophrenics were more responsive than the controls in background physiological activity, largely involuntary, whereas they were less responsive in purposive, selective, voluntary behavior. What does this mean with respect to affective flatness? Probably that the flatness is in the voluntary communication of feelings rather than in the patient's inner experience. Such a conclusion is certainly in accord with the retrospective reports of recovered patients, who seem to recall their psychotic episodes as anything but affectively flat. The McGill investigators interpret their findings in line with the theories of Hebb (25). They suggest that in patients there is a high level of spontaneous nervous activity in inner circuits, showing itself as a preoccupation with ideational processes. This has the effect of preventing external excitations from meshing with ongoing processes; thus it creates difficulty in responding to external demands and interacting with other people.

Although most of this year's work on the organic aspect of schizophrenia stays in the biochemical sector, one report attempts to locate a specific pathology in the brain. Papez & Bateman (54), on the strength of biopsies from 70 schizophrenic patients, describe a series of characteristic abnormalities in the nerve cells, the pattern being much more sharply developed in patients of long standing than in those newly ill. They interpret the findings to signify progressive degenerative changes produced by perceptible inclusion bodies, and these bodies themselves they guess to be minute pathogenic organisms. It is hard to reconcile the hypothesis of degenerative changes with the facts of spontaneous recovery and improvement through different kinds of treatment, but if these findings are confirmed they must be included somehow in our thinking about schizophrenia.

Three papers by Funkenstein and associates carry forward a somewhat different approach to the organic side of mental disorders. Funkenstein, Greenblatt & Solomon (20) previously showed that schizophrenic patients could be divided into several prognostic groups on the strength of their autonomic reactions to test administrations of adrenergic and cholinergic drugs. Meadow & Funkenstein (49) now report that the patients having the poorest prognosis in terms of their autonomic pattern also exhibit, in psychological tests, a relatively marked impairment of abstract thinking. Clinically

this group of patients appears more deteriorated and is classified either as hebephrenic or as paranoid; if the latter, the delusional systems are loosely and poorly organized. The finding lends itself to an interpretation which stresses inefficient action by the cerebral cortex; capacity for abstraction is lowered by this inefficiency and the control of proper firing in the autonomic system is weakened. Funkenstein and associates (22) also report a special study of psychotic patients who exhibit elevated blood pressure. Those whose hypertension is apparently maintained by excessive secretion of an epinephrine-like substance show a good rate of improvement under electric shock therapy, whereas those whose blood pressure is elevated by excessive production of a norepinephrine-like substance make a poor recovery. Finally, Funkenstein and others (21) show that when patients recover, whether spontaneously, through psychological treatment, or through shock methods, their response patterns to test administrations of adrenergic and cholinergic drugs move in the direction of more normal patterns. This makes it seem likely that the less normal patterns found so frequently in the patients represent "different ways in which individuals respond autonomically to stress."

These studies by Funkenstein and associates are still in an exploratory stage, but they contain implications which might well produce substantial changes in our understanding and classification of behavior disorders. They certainly bring comfort to those who groan over the long-lived Kraepelinian categories, for they tend to cut sharply through those categories in a way that would seem to be biologically more fundamental. The patients with elevated blood pressure, for instance, were not all schizophrenics, but the autonomic tests were useful in predicting the outcome of electric shock treatment regardless of diagnostic category. One of Funkenstein's measures, the appearance of anxiety following the injection of epinephrine, predicts, thus far unfailingly, that the patient, whatever his diagnostic category, will be rendered more anxious by electric shock therapy; the effect of insulin, however, is precisely the opposite, reducing the anxiety in these cases. If this work is confirmed and expanded, therefore, it seems likely to bring about an extensive recasting of our thoughts about pathological reaction types, a recasting in which the concepts of stress and anxiety, the newer work on the autonomic nervous system, and the hitherto unintelligible results of the physical therapies will come into conceptual harmony.

AFFECTIVE DISORDERS

During the past year, Bellak (6) has put all students of behavior disorders in his debt by bringing out an encyclopedic review entitled *Manic-Depressive Psychosis and Allied Conditions*. Something like 1200 references are cited and the contents of each is at least briefly indicated, making the book a very satisfactory guide for research workers. Vital statistics, etiology, physiopathologic studies, and treatment are given extensive coverage. In recent years the affective disorders have received relatively little psychodynamic study, although they have been a major target for shock methods of

treatment. Bellak's book reflects this balance of interests, but his own thinking on manic depressive disorders, contained in an introductory essay, is firmly and explicitly psychosomatic.

Bellak entitles his theory not only "psychosomatic" but also "multiple-factor," by which he means that

the clinical conditions referred to as manic-depressive really share only a certain number of phenomena and consist of several widely differing syndromes with different etiological factors.

He believes that the psychopathology of any disorder can be conceptualized under two main headings, the libidinal structure of the personality and the factor of ego strength. Libidinal structure determines the content of what the patient experiences, and for the affective psychoses Bellak accepts in essence the ideas of Abraham and Freud concerning the oral libidinal stage. Ego strength determines the form which the content assumes, whether it appears merely in the dreams of a normal person, in the symptoms of a neurotic, or in the more disintegrated behavior of a psychotic. Bellak here uses ego strength as a quantitative concept, and he assumes that in the manic depressive patient ego strength is greater than in the schizophrenic, less than in the obsessive-compulsive. It is with respect to ego strength that somatic factors become influential. This effect is most evident when a temporary condition such as a toxic state precipitates psychotic reactions in a person previously only neurotic or even normal. Bellak assumes that shock treatments will eventually be understood as having some kind of an effect on ego strength, though he admits that this day seems somewhat far ahead.

The psychological side of affective disorders is discussed in more detail by Rado (56), who adds another paper to his noteworthy series on the psychodynamics of depression. Rado interprets depressive spells as reactions precipitated by real or imagined loss of love which endangers emotional security. There is much in the behavior of a depressed person that can be understood as a cry for love, an attempt to regain the affection and security that has been lost. The reaction unfolds, however, as an expiatory process in which the patient seeks by self-punishment to deserve and obtain reinstatement in affection, the process here betraying its childhood origins in relation to the mother. Complicating the whole sequence is the presence of rage and resentment. At first, the patient is likely to try to vent his rage and force the other person to love him. Failing this, repentance gains the upper hand and the rage becomes directed against the self. Rado's formulation is sufficiently general to fit most schools of thought, although his own background is that of psychoanalysis.

In a small book by Lewin (41), entitled *The Psychoanalysis of Elation*, an attempt is made to rectify the somewhat one-sided preoccupation of psychoanalysts with the depressive aspect of affective disorders. Following other workers, Lewin considers that the mechanism of denial is of central importance in manic behavior. By a rush of activities and interests, and by

an eager flow of speech, the person manages to evade and forget the unhappy features of his personal life. The reaction can hardly be understood, however, without reference to the symbolic meaning of these unhappy features; it is the deep emotional experiences of early childhood, especially those of the oral stage, that require such heavy denial. The elation itself, a false emotion carrying with it an illusory sense of reality, suggests the reanimation of patterns of experience appropriate to an early stage of development: "The feeling of conviction and subjective certainty about the validity of the happy mood repeats an element in the nursing situation." Thus the happy mood is itself a repetition of the "mood at the breast," not a simple result of successfully denying unpleasant current facts. "The illusory sense of reality that is revived in this way is then displaced onto fantasies that contradict current frustrations and deprivations." In this way Lewin applies historical analysis not only to the contents but also to the affective quality of the elated patient's experience.

PSYCHOSOMATIC DISORDERS

The past year witnessed no languishing of interest in psychosomatic medicine. More important than the mere volume of research, however, was a tendency toward increased discrimination in reasoning about the problems of this difficult field. There continue to be reports of the earlier type in which some somatic disorder is correlated with emotional stress, with or without some added feature to account for the selection of the particular organ system. Thus Reiser, Rosenbaum & Ferris (58), studying 12 patients with malignant hypertension, report "close chronologic correlations between the precipitation of malignant hypertension and the occurrence of emotionally charged life situations." They find it possible in each case to relate the precipitating events to "specific dynamic constellations in the patient's total personality," but they shrewdly reach the following conclusion: "The data do not reveal any specificity of personality structure or conflict situations for the group as a whole and thus do not explain the selection of this organ system." They thus state the psychosomatic relationship in its simplest and most modest form: emotional stress—of whatever kind—helps to precipitate somatic illness. In order to explain the kind of illness that is precipitated it is necessary to assume a somatic weakness, in this case probably a predisposing renal lesion.

Two papers attempt to be a little more specific with regard to the choice of organ system. Hinkle, Evans & Wolf (29) in further studies of diabetes mellitus find that the onset and exacerbations occur in periods of acute emotional stress, the remissions in periods of security. They report further that the characteristic pattern of the disease could be evoked by experimental stress. The patients proved to have one characterological feature in common, namely, that overeating was part of their regular response to stress. Both in overt behavior and in autonomic behavior, they responded to stress as if it implied starvation. In this case it becomes less necessary to assume a

predisposing somatic weakness; the somatic disorder bears an intelligible relation to the psychological peculiarity. The situation may perhaps be similar in a study of backache by Holmes & Wolff (31). Their patients were shown to react to emotionally stressful situations by sustained tension in the skeletal musculature. This specific form of overactivity can easily be postulated to build up a concentration of muscular waste products that eventually passes the pain threshold and produces backache.

Elevated blood pressure is a common and presumably innate part of the response to stress. As a result, workers reporting on essential hypertension have been somewhat neglectful in specifying the precise relation between psychological and somatic events. For this carelessness they are vigorously taken to task by Binger (8), who declares:

At present no adequate proof is at hand to establish the fact of psychogenesis or that the commonly observed disturbances of personality are more than frequently occurring associated phenomena.

Binger further cautions against hasty reasoning from transient emotional reactions to chronic ones.

The fact that acute emotional excitement may result in transitory elevations of blood pressure should not be used as a basis for the inference that long-lasting emotional states or conflictive situations can act as precipitants to chronic vasomotor constriction.

There is an increasing tendency to approach psychosomatic problems in a more experimental spirit. Two reports by Jost *et al.* (33, 34) and one by Rupp *et al.* (61) deal with the comparison of autonomic patterns of response in psychosomatic patients and in normal controls. Jost and his associates, starting a long-range program of investigation, report thus far that hypertensives exhibit a greater rise of blood pressure than normotensives, and a slower return to their basal levels, when exposed to a variety of experimental stress situations. The Rupp study deals with an experimental group consisting of 40 male cases of peptic ulcer and utilizes the cold pressor test as a means of applying stress. The autonomic responses of the patients differed from controls by departing more widely from the mean in either direction. The patients tended on the whole to be either over-reactive or under-reactive, whereas the controls occupied the middle range. This suggests that non-specific disturbances or abnormalities in the autonomic regulatory mechanisms might provide a sort of generalized predisposition to psychosomatic disorder. A more basic approach to this whole problem has been going on for some time at the Fels Institute, from which Lacey & Van Lehn (38) contribute a further report. Using a variety of measures including blood pressure, heart rate, and palmar conductance, and selecting the cold pressor test as a stressing agent, these investigators found that children exhibit characteristic individual profiles of autonomic response, and that these profiles are largely reproducible through a second administration of the test. There is a hierarchy of autonomic responses which is stably characteristic

for each individual so that it can be referred to as his "autonomic constitution." This line of research may eventually provide crucial answers to the constantly recurrent question of predisposition.

The full complexity of the psychosomatic relationship is perhaps only just beginning to emerge. A sobering influence upon theory will certainly be exerted by Margolin's dramatic report (44) on gastric functions during psychoanalytic interviews. Margolin's patient in psychoanalytic treatment was a young woman with a gastrostomy, the fourth case of its kind ever reported in American medical history. He confirms earlier findings that different physiological patterns of gastric activity accompany different emotional states. His report goes further, however, and demonstrates that the emotional state of anger itself produces several different gastric patterns depending on the origin, aim, and object of the anger. Fantasied physical violence, for example, went with a synchronized pattern of gastric activity, whereas fantasies of a distinctly oral sadistic character gave rise to a more randomly dissociated gastric pattern. Margolin concludes:

In discussions of psychosomatic etiology, it is a misleading oversimplification to speak of aggression as a dissociated instinctual force without taking into account the libidinal levels with which it may be fused.

The relationship between somatic reactions and emotional states is clearly more complex than it appeared to be in the first blush of enthusiasm for psychosomatic medicine. The simplifications of a pioneer period begin to show signs of having outlived their usefulness. Yet this situation is no different in principle from what happens at every stage of advance in the understanding of behavior disorders. Oversimplification is the price of initial progress. Margolin's paper reminds us not to remain fixated at a level of thinking we should be ready to outgrow.

THE ACTION OF THE BRAIN

Cerebral mechanisms received expert and searching scrutiny in the Hixon Symposium, held earlier at the California Institute of Technology and now brought to publication under the editorship of Jeffress (32). The papers attempt to deal with the most fundamental problems of cerebral function. Both von Neumann and McCulloch, for example, develop the analogy between the brain and a computing machine; Lashley discusses the problem of the serial ordering of behavior as in language and other skilled acts; similarly, Klüver, Köhler, and Halstead undertake to conceptualize the very basis on which the brain performs its multifarious functions.

More directly concerned with behavior disorders is *Frontal Lobotomy and Affective Behavior* by Fulton (19). Dealing mainly with experimental studies of the brain in animals, it nevertheless contains an up-to-date review of the findings derived from lobotomy in man. The forward march of research on lobotomy, reviewed at some length last year by Zubin (75), has brought this form of treatment out of its blindly confident adolescence and into at least

the early stages of maturity. Like the shock treatments, lobotomy can now be applied with some discrimination in different ways to achieve different therapeutic purposes, and it is increasingly possible to avoid those injuries that produce a definite intellectual impairment. The effects of different operations are discussed in detail in a paper by Le Beau (40), who calls attention to the temporary character of some of these effects and suggests that they may be produced in part by operative stimulation rather than exclusion of neural areas.

A substantial contribution to the study of cerebral functions is made by Benda in a book entitled *Developmental Disorders of Mentation and Cerebral Palsies* (7). The book is based on an unusually large population of cases which were studied throughout life and by post-mortem examinations. Benda proposes a radical change in our conception of disorders of mentation. He rejects as totally misleading the common distinction between mental deficiency and mental illness. Only because of an earlier naive reliance on tests of intelligence did we get the idea that idiocy and imbecility were deficiencies merely of an intellectual sort. They should be conceived rather as developmental disorders based in one way or another upon brain abnormalities which affect a variety of functions. The fact of brain abnormality, furthermore, does not signify that psychodynamic processes are of no importance; many of the difficulties exhibited by handicapped children result from their experiences during the course of life. Benda traces many of the disorders of mentation to virus and infectious diseases and nutritional disorders in the mother during pregnancy; these conditions cause certain steps in fetal development to go astray. The cerebral palsies, however, are occasioned by birth injuries, and are of several types each of which is associated with a specific brain pathology. In short, the severer grades of mental retardation have to be regarded as neurological disorders which belong squarely in the realm of neuropsychiatry.

The treatment of the mentally retarded by means of glutamic acid is still the object of research, but the main facts seem now to be fairly well established. Albert, Hoch & Waelsch (1) adopted the plan of giving glutamic acid for limited periods of time with intervening periods when placebos were given. Gains in intelligence test scores were significantly correlated with glutamic acid periods, but the amount of gain was not sufficient to be of practical importance for the patients. Zimmerman & Burgemeister (74) have pursued their investigations long enough to offer findings on the permanency of gains three years after glutamic acid treatment has been discontinued. Some patients hold their gains remarkably well, and for the group as a whole there is a tendency for a significant amount of improvement to be retained. This paper contains a survey and critical review of work with glutamic acid. While it is unlikely that the mental stimulation provided by this substance is sufficient to revolutionize the prospects for retarded children, the mere fact that it has a stimulating effect is of great interest to students of the brain.

The nature and mechanism of convulsive seizures has always been the central problem for physicians studying epilepsy. A further contribution along this line comes from Gibbs (24), who reports that in so-called "psychomotor" seizures the temporal lobe is generally involved as the initial focus of activity. Because of the further finding that cases with temporal lobe involvement exhibit a marked frequency of other psychiatric syndromes, Gibbs speculates that the temporal lobe may be a region of special vulnerability for disorders of various kinds. A more far-reaching speculation is undertaken by Roth (60), who hopes to find a rationale not only for epileptic seizures but also for the convulsive attacks artificially induced by metrazol and electric shock—and for their therapeutic effect. Roth notes that convulsions are naturally produced by a bewildering variety of changes in the internal environment, even presumably opposite changes such as alkalosis (resulting from hyperventilation) and the inhalation of carbon dioxide. He notes also that convulsants are valuable in recovery from the coma produced by narcotic poisoning, and that in some epileptics it is clear that a severe mounting mental disturbance becomes relieved by the fit. To these facts he adds the finding that electric shock therapy seems to have its maximum disturbing effect on the most recent learnings. All of these facts permit the interpretation that convulsive seizures have biological significance, that they are more than a spilling over of excess energy, that they accomplish something. To Roth the fit is a concerted response of the brain "directed toward the restoration of the most stable pattern of cerebral and metabolic functions hitherto attained." It is a kind of natural clearing action when the situation in the brain gets out of hand.

Interest continues to dwell on the effects of changes in cerebral blood flow, a line of investigation greatly stimulated by the recently published work of Himwich (28). An experimental study by Altschule (3) strongly suggests that hallucinosis is favored by a restriction of cerebral blood flow. Drugs calculated to increase the blood flow in schizophrenics have the effect of reducing the force and frequency of hallucinations, whereas decrease of the flow in normal people heightens the vividness of visual imagery for past events. Freyhan, Woodford & Kety (18) conclude that the crucial change in senile psychoses is an increase in cerebrovascular resistance which reduces the blood flow and occasions a significant reduction in the utilization of oxygen by cerebral tissues. Their findings eliminate the old diagnostic distinction between the senile and the arteriosclerotic disorders.

It will not have escaped notice that students of cerebral action, like students of the psychoses, are setting up a clamor against the established categories of behavior disorder. At one end of the life span it is proposed to change the classification of senile disorders; at the other end, changes are suggested in our thinking about the prenatal conditions and birth injuries which have been incorrectly conceptualized as mere intellectual retardation. There is clearly a general trend toward what may be a very considerable revolution in our way of classifying abnormalities of behavior, in our way

of thinking about them, and even in our way of treating them. One hope in this possible revolution is that if we can arrive at more basic biological reaction patterns and also at more basic psychological reaction patterns these two classes of events will prove at last to be truly convergent. If this should happen, our present act of faith in omitting the hyphens from such words as "psychosomatic" and "biosocial" would be justified by the state of knowledge.

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CLINICAL METHODS: PSYCHODIAGNOSTICS^{1,2}

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The absence of any generally accepted personality theory and the primitive development of the theories that are utilized by those clinicians who feel the need for a rational explanation of their activities continues to result in a situation where experimental investigation of test validity emphasizes more and more the absence or inefficiency of prediction. This is true, at least, when the test or technique is to be used as an objective scientific instrument rather than as a personal one. Some of the inconsistency in experimental investigations and the inadequacy of prediction for clinical purposes may be related to variations and weaknesses in the experimental designs. It seems more likely, however, that these findings are the result of the intrinsic defects in the tests which have been developed. These tests are either unrelated to explicit theory (and, consequently, unrelated to the large body of experimental data we have accumulated in the study of the psychological reactions of organisms) or are loosely related to nonoperationally defined theories which have yet to weather experimental test.

There are, of course, many studies which show some validity for some instruments of personality assessment. Until better theories are devised and instruments are developed specifically as ways of measuring constructs which have experimentally tested predictability, the clinician will have to pick and choose, as best he can from the current variety of tests, those instruments which seem to fit best his own theoretical orientation and purposes. Tests which have so far failed to show predictiveness when subjected to objectification and quantification may still provide a maximum of information when used as a personal instrument in relationship to many nonquantified variables, but they might well be rejected when one is seeking an instrument to be used to test an hypothesis in an experimental investigation.

This review will consider the contributions of the last year to the problem of psychodiagnosis under three broad headings: (a) general contributions to the methodology and theory of personality measurement, (b) clinical instruments, and (c) research instruments. Distinction between the second and third categories will be on the basis of their primary use. Some studies using diagnostic tests, but concerned primarily with testing hypotheses regarding the characteristics of abnormal groups or the status of personality theories, are treated in other chapters.

¹ The survey of the literature to which this review pertains was completed about May, 1952. The author wishes to acknowledge the valuable assistance of Mr. Harold M. Schroder in the preparation of this review.

² The following abbreviations are used in this review: TAT (Thematic Apperception Test); MMPI (Minnesota Multiphasic Personality Inventory); WISC (Wechsler Intelligence Scale for Children); MAPS (Make a Picture Story).

GENERAL CONTRIBUTIONS

In last year's review of the field of psychodiagnostics, Magaret (66) emphasized the relative paucity of contributions on methodology or of tests of general theoretical principles underlying one method of diagnosis (or personality description) as opposed to another. Unfortunately, the situation has not changed greatly in the past year. The latest book on projective methods, edited by Anderson & Anderson (6), contains several of the more important discussions of general problems. Macfarlane & Tuddenham's (65) discussion of problems in the validation of projective tests emphasizes the extreme difficulty of experimentally testing the validity of a test as a measure of the "whole" personality or the "basic" personality and suggests rather a development of a number of tests, each designed to serve a narrow function. They suggest also a resolution of the problem of global ratings versus specific scoring categories by having specific scores but weighting them as in multiple regression equations to predict quantitative criteria.

Cattell's (19) discussion of the design of "projective or misperception tests of personality" in the Anderson & Anderson book provides several provocative suggestions. He classifies approaches to personality assessment into three broad categories of observation. These are: (a) life record data (observations *in situ*), (b) self-rating data (questionnaires, etc.), and (c) objective test data. The latter infer personality from actual behavior in a defined, reproducible, artificial or laboratory situation. It is in this latter group that he places the projective tests (rightly pointing out that this title is a misnomer) and emphasizes that these tests require the same objectification, comparison to norms, and external validation as do other tests. They are, in fact, tests of distortion or misperception (deviation) from a norm or a standard of reality. Tests measuring the distortion resulting from dynamic needs (rather than cognitive problems) are dynaception tests. Cattell illustrates some of the tests used to measure the factors basic to his theory of personality. Although one may disagree with Cattell's approach to personality description, the systematic designing of measuring instruments to provide operations for the basic constructs of a systematic theory (in Cattell's case, a factor-trait theory) is indeed a real step forward in providing methods of personality study with an understandable rationale that can lead to clear-cut studies of utility.

Henry's (49) chapter in the Anderson & Anderson work (6) provides some important developments in the use of projective tests of the TAT² type, in the analysis of small groups, and for cultural analysis. In the study of cultural groups, Henry describes a method of analysis wherein the stories for a large group are treated as a kind of unit and analyzed as such instead of as a series of individual analyses put together. That is, the stories of the entire group for a particular picture could be analyzed as a unit, then stories for the next picture, etc. The individuals would retain no identity in this approach. Such a method utilizes the test not merely as a way of measuring individual (*i.e.*, psychological) variables but also broader sociological or cultural variables.

He states, however, that in order for the method to be applicable, the group must be of consistent social characteristics and be of highly similar psychological structure. It is difficult to perceive, in light of cultural relativism, how one can determine such consistency and similarity.

In addition to the chapters mentioned earlier, Anderson & Anderson (6) contains 21 other chapters dealing with a variety of projective methods in common and not-so-common use. These chapters vary from critical reviews of research and development to highly uncritical expositions of a particular technique's potential to get at what is really important in the study of personality. Several chapters are primarily concerned with the straightforward description of a particular method of test administration and analysis.

Although partial reports of the Michigan assessment study of Veterans Administration trainees in clinical psychology have been appearing in the literature during the last few years, an integrated report of the full study has been published only recently by Kelly & Fiske (57). Several of their findings and conclusions have broad implications for the field of psychodiagnosis. In general, their findings question the predictive value of clinicians operating from global impressions, artistic or personal methods, and the predictive value of projective tests in general. Although prediction against criteria of success in the training program was low for all kinds of measures, simple and objective measures did as well or better than the more uneconomical and personalized instruments. This study perhaps is one of the most elaborate and thorough attempts to put the state of knowledge in a particular field to a practical test, and an analysis of the meaning and limitations of the results should be of great interest in pointing the way to new developments. As to limitations in generalizing from this study, it should be remembered that the group was a highly homogeneous one, the individuals were not naive in regard to the methods by which they were being assessed, and the criteria for competence and success were admittedly weak. In addition, the raters and clinicians had no specific knowledge at the time of rating of how the criterion, "success in clinical psychology," was to be measured, nor were there systematic attempts to reach any agreement among the raters as to the ideal criterion. An overview of this study leaves this reviewer with two general impressions. One of them is the striking lack of relationship between the "experts" confidence in themselves and their methods (even in situations and problems where they had never tried these methods before) and their actual ability to predict. It re-emphasizes the inadequacy of the practice of approaching test validity through "clinical experience" or "expert" pronouncement. The second general impression is the inherent inefficiency of approaching a problem of psychodiagnosis without some systematic theoretical orientation. In this case, there was frequently little in common between the theoretical orientation of the clinician-raters, the theoretical assumptions of the tests they used, and the theoretical assumptions on which the criteria were determined. In many instances, the raters were emphasizing subtle personality characteristics, but it seems likely that intellectual ability was highly

weighted in the criteria variables. It is not difficult to explain much of the inefficiency of prediction on these bases.

Rosenzweig (82) has characterized the basic assumptions of psychodynamics and projective methods that differentiate them from the general-experimental (universal laws) and the statistical (group norms) approaches to personality. These are: response dominance or emphasis on response-response relationships, configuration dominance or the subordination of the part to the whole, and idioverse dominance or analysis of the individual's behavior from the individual's standpoint. He has also discussed the implications of these assumptions on traditional concepts of validity and reliability. The major merit of this discussion appears to the reviewer to make explicit what for some has been implicit, and it should help the intrepid few who are seeking to develop a theory or rationale of why and how projective methods are of value in the prediction of human behavior.

An article of major importance in the field, although restricted to the TAT, is Lindzey's (61) analysis of underlying assumptions in the TAT and experimental evidence (taken not only from research with the test but also from other fields of psychology) that can be used to confirm or reject them. This type of analysis not only points the way to the important research problems that require investigation in regard to test validity but also, if done before new tests are reported in the literature, might result in the early rejection of many of them, thus saving the profession the laborious process of mastering the esoteric techniques of analysis and trying them out a couple of years in order to prove they lack any reasonable clinical utility.

One such test of a general assumption underlying some diagnostic methods was made by Eriksen (30), who showed that the threshold of recognition was elevated for the recognition of pictures illustrating "unacceptable needs." Implications of the operation of such "perceptual defense" are significant for methods of test analysis which give importance to distortion, delay, and omission in the perception of test materials.

A trend of general significance also relating to projective tests is represented by the studies of Weisskopf & Dieppa (106) and Gibby (37) which, taken along with a series of previous studies, indicate the susceptibility of these methods to suggestion, faking, and examiner influences.

Palmer (73) contributed a general methodological study of the relationship between two methods of ascertaining test validity. Using the Rorschach, Palmer used psychoanalytically oriented therapists' knowledge of their patients as a criterion, and compared two methods of validation—matching of test interpretations (the therapist had to select his own case out of a group of five controlled for heterogeneity) and comparison of multiple-choice check list judgments made independently by the interpreter and the therapist. He found low validity for the matching method ($C=.43$) and practically no (zero) validity for the objective check list approach. The cases that were correctly matched in the matching experiment did not show any greater validity in the check list method than those that were not correctly matched.

Palmer chooses to interpret these findings as indicating the inadequacy of the check list approach, apparently assuming the validity of the test. He points out that this inadequacy is present because of the fact that it is extremely difficult to describe the behavior of the individual in the test and life situations by a common set of statements. The reviewer would ask, however, if this is so, what is the purpose of the test? Implications of this study for the validity of the Rorschach test will be discussed further in another section.

Perhaps one of the most hopeful results of a survey of the literature for the year is to perceive the increasing recognition of the poor state of theory and the recognition of the special problems of validation [Hutt (56), Hertz (50), Macfarlane & Tuddenham (65)]. There is concurrently a growing dissatisfaction with current tests and a tendency of new texts, exemplified by Watson (103), to recognize the limitations of instruments, to stress the problem of economy, and to break down the notion of a "true" test validity by substituting the concept of many different validities for different purposes.

CLINICAL INSTRUMENTS

PROJECTIVE TESTS

As usual, most of the published research of the past year on diagnostic or personality measurement has dealt with that loose collection of techniques usually designated as projective methods. It is difficult to say, with so little perspective, whether or not projective tests are still booming or are beginning a decline to a less prominent place in the clinician's armamentarium. Certainly, studies pointing out limitations and lack of validity are increasing, but interest in these instruments is still high; and there is little evidence that clinicians are ready to return to the personality inventories to the degree they once depended on them. Before projective tests will be abandoned they will have to be supplanted by other instruments that can quench the clinician's thirst for methods providing information about the dynamic or motivational aspects of his subjects. Unfortunately, the need of many clinicians to find out all that is important about the subject by the magic sampling of some small segment of behavior will only be eliminated by training a new kind of clinician (a process which at least has made its beginnings in many graduate schools).

Rorschach studies.—The Rorschach Test continues to maintain a dominant place in the research and clinical application interests of clinical psychologists. Numerous validity studies have been reported in the past year. Although it is the author's impression that the Rorschach in clinical practice is being used more and more in an impressionistic way with less and less regard for the scoring or "psychogram," few reports have appeared relating to its systematic use for content analysis or for its systematic application to a psychoanalytic framework. Beginning attempts in this direction have been suggested by some by the designation of certain cards as the

"father" or "mother" card and by use of conventional psychoanalytic symbolism. Palmer's (73) study, described earlier, bears on the use of the Rorschach in this way. Extremely low reliabilities were found in having four experienced Rorschach interpreters make check list judgments using 34 variables. The author found that comparatively higher reliability was obtained on items covering intellectual functioning and control, and very low reliabilities on items covering sexual attitudes and types of anxiety defenses. There was no significant relationship between these ratings and similar ratings made by psychoanalytically oriented therapists on their own patients. Rosen (81) asked his subjects, male and female college students, to select the cards which were most like the father symbol, a mother symbol, male sexuality, and a family symbol, along with other questions. He found considerable support for the hypothesis that under such conditions card IV is seen as a father symbol, VI as representing a male sex organ, and VII as a mother symbol, but not for X as a family symbol. However, great individual differences were also obtained; and no single meaning was assigned to any specific card by a majority of the group.

Studies of the conventional scoring of the Rorschach have been numerous. In regard to over-all studies of validity, Wittenborn & Holzberg (112) failed to find anything but "tendencies" in a comparison of Rorschach scores with psychiatric diagnoses of 199 successively admitted mental patients. Using a more controlled procedure than the regular Rorschach procedure, McReynolds (67) showed that highly heterogeneous groups of normals and patients showed over-all differences in their means. The groups, however, were so heterogeneous (including feeble-minded, paretics, psychotics, and college students) that it would be hard to conceive of any testing procedure or measure (including age, height, weight, and socioeconomic status) which would not show significant differences in over-all means.

Factor analyses of the Rorschach have been done by Cox (24) and by Adcock (1). However, these have used very heterogeneous groups and there is little relationship between their factors or consistency with previous factor analyses, except for the presence of a factor of productivity with its highest loadings on R. They also include a factor of intelligence which overlaps with productivity.

New normative material has been offered by Cass & McReynolds (18) (58 normal adult men and 46 normal adult females), using Klopfer and Kelley scoring along with Beck's scoring for form level, and by Sanderson (84) who determined norms for reaction time for 50 adult clients of a vocational guidance service. Neff & Lidz (71) compared the Rorschachs of overseas soldiers, considered normal, of different intelligence levels and stated that only the group of superior intelligence approximated standards usually considered adequate for normal individuals. Groups of lower ability would be considered neurotic according to the diagnostic sign approach.

Barry, Blyth & Albrecht (8) compared test and retest data on the Ror-

schach, with pooled judgments of adjustments of patients at a Veterans Administration Mental Hygiene Clinic. Hypotheses that changes in ratings of adjustment level would be correlated with changes on the Rorschach were not supported statistically, although some relationships between ratings of adjustment level and the Rorschach indices at the time of the retest, but not the initial test, were found to be significant.

Other general reports on the Rorschach include an article by Wittenborn (111) suggesting that difference scores are not economical and do not add to prediction, further comments by Eichler (27) on the need to control for differences in the total number of responses in interpreting other scores, a study of Alden & Benton (3) showing no significant difference in the number of sex responses obtained by a male and female examiner from 50 male hospitalized patients, and a review by Hertz (50) which thoroughly discusses the current problems of the use and validation of the Rorschach. Roe (80) has published a summary of her studies of university faculties with the Group Rorschach. The relationships between *R* and all other determinants were quite high and constitute another indication of the methodological weakness in the use of raw scores for Rorschach determinants. Eichler (28) found in a comparison of Rorschach and Behn-Rorschach that reliabilities, although approaching Rorschach test-retest reliability, were too low for individual use. His general conclusion was that the Behn was a similar, but in no sense parallel, test to the Rorschach. Sanderson (85) has suggested a method of extending the testing of the limits by asking the subjects to give a title for each card.

In the studies of specific aspects of Rorschach interpretation, color has, as usual, come in for much investigation because of its relative susceptibility to experimental manipulation. Allen, Manne & Stiff (5) did not find significant differences with screened "normal" students in color shock indices for chromatic and achromatic cards. They did not use a control (in this case, maladjusted group) and previous studies have shown that similar results would probably be found on a maladjusted group. In this same study they found as much consistency on noncolor cards as on cards presented once with color and once achromatically. Consistency was defined as identical response to the same area in test-retest performance. These results suggest the startling conclusion that color does not lead to any significant differences in response.

Gardner (35) found some indicators of impulsivity promising and others not holding up. This study, however, is primarily suggestive since it utilized only 10 subjects and allowed for some bias to enter into the administration and scoring of the Rorschachs.

Other studies utilizing achromatic cards include one by Perlman (75) on the 8, 9, 10 per cent. Like previous studies, this one found that the number of responses on cards 8, 9, and 10 is not a function of color and the 8, 9, 10 per cent cannot validly be considered a color index. Buker & Williams

(16) found that, in general, the presence of color does not materially influence responsiveness of male adult schizophrenics to the Rorschach. However, the presence of color did significantly increase reaction time on the color cards. Siipola, Kuhns & Taylor (92), in contrast to Allen *et al.*, did find color to significantly influence consistency or number of identical responses. Using a somewhat different design, they determined the number of identical and of different responses in groups with two presentations of the achromatic cards versus groups presented once with the chromatic and once with the achromatic series. Significant differences were found. Although concluding that an individually oriented technique of measuring the influence of color was feasible, they remark that the current clinical method of assigning color scores indicating different degrees of color influence is unrealistic. Hughes, Epstein & Jost (54) studied the relationship between autonomic nervous system functions associated with affective tone and color responses on the Rorschach. No significant differences were found on any of a variety of such measures for color responses versus noncolor responses. These results, however, suggest little since one might expect disturbance in these functions on a number of responses other than color responses according to Rorschach rationale. Fonda (33) studied the validity of white space responses by comparing them to the frequency of responses in which subjects chose to answer personality questionnaires by circling the "questionable" alternative instead of either "yes" or "no" and by relating the white space responses to test scores (on the Guilford-Martin) to factors purported to measure negativism or its opposites. After converting the frequency of white space responses to standard scores to control for differences in *R*, he found no relationship to the inventory factors but a low (.38) and significant correlation with frequency of "?" responses. Although such findings might be considered as weak support for the usual interpretation of white space responses, it must also be admitted that other possible variables relating the two measures need to be investigated. Test-retest reliability of the white space response for the standard cards in the Harrower-Erickson series was .58 for main *S* responses, and .18 for additional *S* responses.

Anxiety indicators have been investigated by Eichler (26) who subjected an experimental group to stress before Rorschach administration and did not so treat a control group matched on the Behn-Rorschach. Evidence of the effectiveness of stress was demonstrated in increased errors on a subtraction test during which shock was administered to the experimental group. Of 15 indicators studied, four showed significant differentiation of the group in the hypothesized direction. These were: increased number of weighted shading responses, reduced total responses, reduced whole responses, and increased number of oligophrenic details. A fifth, increased pure form responses, was significant in the wrong direction and three others tended toward significance. The author suggests the remaining indicators may not be valid or perhaps are not sensitive to this kind of anxiety. Perhaps some of the

failure in the ability to test many aspects of the Rorschach is due to the absence of definition of the concepts employed in general behavioral terms which allow for logical objective criteria.

Approaching the problem more from the point of view of process or general approach to problems, Klein & Schlesinger (59) demonstrated that "form boundedness," rated from the Rorschach by consideration of both quantitative and qualitative aspects of the Rorschach, related to the range of apparent movement experienced. The authors feel that such an hypothesis is meaningful in terms of a broader theory of ego functioning. This attempt to recast the Rorschach into a more integrated theoretical approach than is now the case represents a significant and provocative approach to the problem of Rorschach validation. Perhaps in its broader implications it is a similar approach to the problem of Rorschach validation from a semantic or language usage orientation. In such a study Hays, Gellerman & Sloan (48) were able to establish some predicted relationships between verb-adjective quotient obtained from an independent verbalization sample and Rorschach scores.

Harrower & Steiner (45) published a revision of their *Large Scale Rorschach Techniques*. It reviews some new studies and maintains an optimistic attitude toward the Multiple Choice Test somewhat out of keeping with the many negative reports of the use of this instrument.

Thematic Apperception methods.—Lindzey's analysis (61) of the validity of the assumptions underlying TAT methods, described earlier, not only serves as an assessment of current validation status but also is suggestive of several areas of fruitful research with the TAT. One experimental test of underlying assumptions of the TAT was made by Ericksen (31), who related the recognition threshold for aggression on eight drawings to the frequency and characteristics of response of aggressive themes on 10 TAT cards. He found a correlation of $-.46$ between recognition thresholds and the number of stories in which aggression was the main theme. That is, subjects who tended to see aggression in the drawings told more aggressive stories. He also found that subjects who did not see aggression in the drawings (high threshold) revealed aggression by blocking, distortions, etc., on TAT cards normally producing aggressive themes.

A number of studies of the TAT are related to problems of administration and the significance of different stimulus material. Weisskopf & Dieppa (106) demonstrated that instructions to make a "bad" impression and to make a "good" impression produced significant differences in the rating of judges made from three stories on variables such as adjustment and hostility. Implications of the study are that faking or conscious attempts to make a particular kind of impression are possible. The subjects were 24 male, hospitalized veterans diagnosed as psychoneurotic. Bijou & Kenny (12) tested the ambiguity of TAT cards, preparatory to a study of the revelatory value of cards of differing ambiguousness. They found that subject ratings

of the ambiguity of pictures did not correspond too well with Murray's division of the cards into two series, the second supposedly more unstructured than the first.

Schwartz, Reiss & Cottingham (89) made an analysis of "idea counts" as well as word productivity reported in an earlier study to demonstrate that Thompson's TAT with Negro figures does not, with either white or Negro administrators, produce more material with northern Negroes. Thompson & Bachrach (98) made modifications on both the TAT and the Thompson TAT by coloring the cards. They found that the colored cards produced more words and more affective material. They did not describe their controls for possible bias in the scoring of the affective variables.

Schneidman (87a) has edited a book on clinical approaches to the TAT in which a group of "experts" have applied their methods to a blind analysis of the same TAT and MAPS test protocol.

Reports of other instruments similar to the TAT include articles of clinical application of the Make a Picture Story by Goldenberg (39) and Walker (100). Two reports by Andrew, Walton *et al.* (7, 102) on the Michigan Picture Test deal with preliminary data obtained on relatively small samples. This test shows promise for the screening of maladjusted children in the 8- to 14-year range.

Sanford & Rosenstock (87) describe the use of seven projective pictures of the cartoon type in door-to-door interviewing and feel that the results are promising for such an application of projective methods.

Szondi Test.—Whether as a function of esoteric interest value, clinical interest in its efficacy, or because some clinicians can more readily see the inherent theoretical weaknesses in the Szondi Test than other projective instruments, this test has produced more research studies than all but a few of the projective methods. In regard to validity of underlying assumptions and diagnostic value of the instrument, the studies by Guertin (42, 43), Fosberg (34), Wallen (101), Mussen & Krauss (70), and Lubin & Malloy (63) generally agree in challenging the assumptions regarding the equivalence of pictures presumed to be in the same "vector," which is the basis on which subjects are classified, and in showing relatively poor or insignificant value in separating patients of one diagnostic category from another. Sandler & Lubin (86) were able to demonstrate adequate test-retest reliability.

Bender-Gestalt Test.—Suczek & Klopfer (96) attempted to arrive empirically at the meaning or associative value of the Bender-Gestalt figures by having subjects free-associate to the figures under controlled conditions. Although this method presents difficulties in arriving at established meanings for different groups of subjects, it is undoubtedly superior to having the clinician arbitrarily, or on the basis of limited clinical experience, designate one figure as a male sex organ, another as a mother figure, etc.

Hanvik (44) found that three different methods of protocol analysis, including over-all sorting by four judges, failed to distinguish 23 patients

with functional back pain from 14 patients with proven organic disease of the back.

Pascal & Suttell (74) have published a manual which describes a method for objective scoring of the Bender-Gestalt as well as a clinical method of interpretation. Validity studies indicate that the over-all objective scores discriminate patients from nonpatients adequately but not neurotics from psychotics. The test score also has prognostic value in predicting hospital improvement. It is not clear from the manual, however, whether these validity tests were done on a new population or were part of the same population on which the scoring method and weights were determined.

Bensberg (10) compared the Bender-Gestalt protocols of 161 mental defectives with an equal number of defectives of the familial type, matched for mental and chronological age. Three of eight measures studied significantly differentiated the two groups. The relative successfulness of this type of use for the Bender-Gestalt, as compared with its use in personality description in the dynamic sense, appears to the reviewer to fit the current data on the specificity and generality of response.

Incomplete sentences tests.—Meltzoff (68), in an elaborate study of some of the assumptions in incomplete sentence tests, demonstrated the relationship between the tone of the stems and the responses, as has been done for the TAT. Neutral stems provided the greatest freedom of response. He also found predicted differences in responses as a function of instructions which asked subjects to simulate good and bad adjustment and instructions which stated the results would be turned over to people in authority, as compared with ones which asked the subject to remain anonymous.

Figure drawing.—Berman, Klein & Lippman (11) report some observations regarding differences in patient types and drawings, determined by discussing cases with the psychiatrist.

Mosaic Test.—Shotwell & Lawrence (91) report several aspects of significant or near significant differences in the test-performance of brain-injured feeble-minded compared with undifferentiated defectives, matched on CA, MA, and IQ.

Rosenzweig P-F Test.—Spache (93), using the Children's Form of the Picture Frustration Test, failed to find significant differences in the test as a whole in the type or direction of aggression in boys as compared with girls. Boys did, however, show more outward aggression toward other children than toward adults. Taylor & Taylor (97), working with the Group Conformity Rating, found in their population of 130 adult males that variance among items was significant but variance among individuals was negligible, and concluded that the Group Conformity Rating is not a reliable measure of any trait. Holzberg & Posner (53) failed to find a relationship between scores of extrapunitiveness and socio-metric ratings, TAT judgments, the Allport Ascendancy-Submission Scale, or supervisor's ratings.

Wertheimer & McKinney (108), perhaps in support of the notion that

any kind of material may be approached from a projective point of view (i.e., that it is the method of analysis rather than the form of the materials that characterizes projective techniques), utilized a case history blank as a projective technique. They were able to differentiate clinically classified college students on such variables as use of extreme ratings, use of space (i.e., writing between lines), feeling tone, etc. It seems to the reviewer that such emphasis on generalized ways of reacting to the total meaning of the situation with more emphasis on the meaning of the behavior and less on the specific reactions to individual figures, cards, etc., may indeed pay bigger dividends than emphasis on responses to supposedly unstructured stimuli which may be highly structured and call forth quite specific reactions. In other words, one might better ask, "How did he react to the test?" than, "What did he do on Card IV?"

OBJECTIVE TESTS, PERSONALITY MEASURES

With the exception of work on the Minnesota Multiphasic Personality Inventory, there has been relatively little published regarding the use or validation of objective measures of personality. This trend is perhaps not so much a function of poorer validities found for such instruments but is due to the fact that any given instrument yielding only a deviation from a norm on one or a few variables gives the psychologist relatively little useful information and little opportunity to exercise his own creative powers.

One study of general interest is that of Elias (29). Elias gave Maslow's General Health and Hygiene Inventory to a large number of college students and followed it several weeks later with Maslow's Security-Insecurity Test. Results supported his hypotheses that (a) extreme cases on the Security-Insecurity Test would differ from each other in that the insecure students would answer "yes" more often to statements describing complaints of any kind, (b) they would use the "?" answer more often, and (c) the more unstructured the item (i.e., did not refer to a health condition, such as scarlet fever which is usually diagnosed only by a physician) the more it would differentiate between secure and insecure testees. This study agrees with the report of Wertheimer & McKinney (108), cited earlier, in placing significance on generalized attitudes of taking personality tests and should be of value in considering methods of scoring such instruments.

The Minnesota Multiphasic Personality Inventory.—Tyler (99) factor analyzed 15 MMPI² scales of 107 college women and found five factors which he feels conform with the way clinical psychologists interpret the MMPI. Agreement with previous factor analyses is not very clear except for a general factor found in most studies with heavy loadings indicating high interscale correlation. Clark (22) found that the MMPI scores of three groups of army general prisoners were all characterized by elevations in *Pd* and *Ma*, and general elevation on all scales as compared with Schmidt's normal group of soldiers. The three groups diagnosed as "no neuropsychiatric

disorder," "emotional instability," and "antisocial personality" showed very little differences between groups except in the over-all height of the profiles. Hathaway & Monachesi (47) also report the use of the MMPI in the diagnosis or prediction of delinquency. They did not report statistical findings, but apparently were able to find only slight trends in predicting the appearance of delinquency two years after the MMPI was given to a large group of ninth-grade children. Wiener (109) found that Subtle and Obvious item scoring on the MMPI differentiated hospitalized from non-hospitalized schizophrenics—the hospitalized giving higher Obvious scores, the nonhospitalized, higher Subtle scores. Comparing these findings to previous ones of successful and unsuccessful salesmen, he concludes that a general factor of social control may be measured with these keys. This study is suggestive that these keys, rather than the unsuccessful *K* scale, might be used to refine the diagnostic implications of the MMPI. Cauffiel & Snyder (20) studied the relationship of the Psycho-Somatic Experience Blank with the MMPI. They found moderate positive correlation with all the scales except *K* which had negative correlations, and that the correlation with the *Pt* scale was substantial.

Two new scales of the MMPI for Dominance and Social Responsibility were reported by Gough, McClosky & Meehl (40). Neither scale has been cross-validated on subjects other than the criterion group. Welsh (107) has described an Anxiety Index and an Internalization Ratio for measuring anxiety on the MMPI. Feldman (32) constructed a prognostic MMPI scale (*Ps*) for improvement for shock therapy. He found excellent separation of recovered and unimproved cases in both criterion and test groups. Recovered cases, however, tended to follow diagnostic lines so closely that it remains to be determined whether or not the test will differentiate adequate samples of the same diagnosis who improve or do not improve under shock therapy.

Spence & Taylor (94) and Hilgard, Jones & Kaplan (51) have studied the relationship of scores on the Taylor Anxiety scale of the MMPI to conditioning of the eyelid response. Both studies indicate that subjects selected as extreme on this scale can be differentiated in the "threatening" type of learning situation characterized by the eyelid conditioning situation.

Shepler (90) compared three objective tests of masculinity-femininity with a new projective test of masculinity-femininity, the Franck Test. The objective tests were the Terman-Miles M-F Test, the MMPI, and the Strong Vocational Interest Blank. All four scales significantly differentiated men from women college students—the Franck Test, least efficiently. All three objective measures had significant intercorrelations but the Franck Test did not correlate with any of the others, indicating, perhaps, that this test was measuring a different aspect of masculinity-femininity than the other tests.

Difficulties in tests validated on psychiatrically diagnosed disease entities are well illustrated by a study of Gilliland (38). Gilliland correlated five

scales on the Humm-Wadsworth with five scales on the MMPI which, by scale name and description, should be measuring approximately the same thing. He found, however, that with six different samples, only a chance agreement was obtained between the two scales. Correlations of the two tests with a mean of three ratings on each of the five personality variables were also obtained. With both tests, correlations were predominately positive but quite low—the MMPI showing most agreement on the psychopathic deviate and depression scales, the Humm-Wadsworth, on the schizoid and depression scales. Four of the six groups were given the group form of the MMPI—two groups, the individual form. The groups with the individual form showed no better correlation with the ratings than did those given the group form.

Behavior rating scales.—Both Rowell (83) and Lucero & Meyer (64) have reported the use of rating scales for the determination of psychotic behavior of patients in mental hospitals. Reliability of both scales appears good and these scales may be of considerable value for matching subjects for experimental purposes as well as for clinical determination of mental status. Lorr, Wittman & Schanberger (62) report a factor analysis of the Elgin Prognostic Scale. They obtained three oblique factors which they labeled schizoid withdrawal, schizophrenic reality distortion, and a less well-defined factor of personality rigidity or inadaptability.

Wittenborn & Mettler (113) correlated nine symptom clusters, previously arrived at by factor analysis, with 61 behavior rating scales appropriate for occupational therapy situations. Of the 549 possible relations, only 45 were significant at the .05 level. With such low predictability, it appears to the reviewer that factor analysis of symptom descriptions from the same disordered theoretical approach as characterizes present day psychiatric practice is hardly a promising solution to the problems of prediction and control of abnormal behavior.

Studies of intellectual functioning.—A paper of general interest in this area has been contributed by Weisskopf (105) who reviewed dynamic personality factors influencing intellectual performance. Weisskopf makes an impressive case for the need for more systematic research relating intellectual functioning, particularly malfunctioning, with the total personality. Her emphasis is not on predicting personality types from profiles, scatter, etc., but rather on understanding intellectual malfunctioning as a predictable outcome of certain dynamic attitudes of subjects.

Beier (9), using a Rorschach interpretation as an anxiety producing situation, showed that tests of abstract reasoning—the Holsopple Sorting Test and a mirror tracing test—were negatively affected in an experimental, but not in a matched control group. Implications for the relationship of the anxiety condition of the patient to test performance under different conditions of testing are obvious.

Two articles have appeared on the use of an altitude score for psycho-

metric diagnosis. Such a score determines in a variety of ways the discrepancy between the best performance and the mean performance for a scale of several subtests. Purcell, Drevdahl & Purcell (77) found low (r 's of .24 and .31) relationships with three scales of the MMPI which they characterize as anxiety scales. Diller & Beechley (25) demonstrated that an altitude measure on the Stanford-Binet is stable from test to retest. One measure of altitude frequently employed is discrepancy between vocabulary score and overall performance. Chodorkoff & Mussen (21) studied the significance of the apparent stability of vocabulary scores in mentally disordered patients. They compared schizophrenic and normal patients, matched on a choice vocabulary test, on a second test of vocabulary in which they were to select the best of several correct alternatives. They found that schizophrenics, more often than the normals, selected alternatives of a lower conceptual level (i.e., function and example as opposed to class) and conclude that vocabulary performance does change qualitatively in mental disorder.

Signs of progress are apparent in the absence of new methods of differential diagnosis by comparing profiles or patterns of subtests of the Wechsler-Bellevue. Rabin & Guertin's review (78), and also a more recent one by Schofield (88), have summarized the difficulties and consistent negative results of this approach. Another negative report of the use of such patterns was made by Alderdice & Butler (2), who tested Wechsler's diagnostic signs for feeble-mindedness. Alimena (4) has described a method for equating subtests for analysis of scatter to overcome the limitations of Wechsler's original norms.

Block, Levine & McNemar (14) recommend a method for testing the differences between groups on psychometric patterns or profiles from the Wechsler-Bellevue by using an analysis of variance method and the interaction term. Related to such scatter analyses are hypotheses regarding the psychological significance of the subtests on the Wechsler-Bellevue. Holzberg & Belmont (52) made 45 predictions regarding relationships between the Rorschach and the Wechsler-Bellevue based on the rationale for the various submeasures on both tests. Only four predictions were found to be statistically significant. The obvious implication of this study is that the rationale used for the Wechsler subtests, the Rorschach scores, or both tests is obviously highly invalid.

A thorough study of the relationship between the new WISC² and the Revised Stanford Binet has been reported by Krugman *et al.* (60). Correlations between the Stanford Binet and the WISC were .74 for the verbal scale, .64 for the performance scale, and .82 for the full scale. In general the Stanford Binet yielded higher IQ's. At the higher IQ levels, the difference was most marked. Most of the discrepancy is a function of the difference between the Stanford Binet and the performance scale of the WISC.

The usefulness of intellectual tests to differentiate brain-damaged cases from nondamaged was demonstrated by Winfield (110) who compared epilep-

tics with demonstrable brain damage to those with no demonstrable brain damage. He found that a battery of tests, including the Progressive Matrices Test, the Wechsler-Bellevue Digit Symbol, Associate Learning on the Wechsler Memory Scale, and the SRA Primary Mental Abilities Test, all successfully differentiated the groups. These results support the frequently validated principle that in brain damage it is new learning which primarily appears to suffer.

Zubin & Windle (115) report a follow-up on the use of the Metenym Test for prognosis of psychosurgery cases and their controls. Although their results are promising, even if difficult to explain, a cross-validation of their scoring methods is indicated before these results might be considered stable.

Pollaczek (76) tested the hypothesis that malingerers failed both hard and easy items whereas the true mentally deficient failed primarily the hard items. Using the CVS Abbreviated Individual Intelligence Scale, she was able to detect 90 per cent of subjects instructed to simulate feeble-mindedness, while only falsely identifying 10 per cent of the mental defectives. Detection was not possible on the basis of over-all scores.

Hunt & French (55) have published preliminary data on a nonverbal test called the Navy-Northwestern Matrices Test. Diagnostic potentialities between normals and patients including paretics, schizophrenics, and mental defectives are indicated.

Stacey & Portnoy (95) report no differences on an object sorting test in differentiating morons and borderline subnormals in the adequacy and span of the sortings but felt on further analysis that some qualitative differences emerged.

New norms on two performance tests, the Lincoln Hollow Square and the Kohs' Color Cubes, based on 600 children in Vermont, have been published by Kent (58).

EXPERIMENTAL MEASURES OF PERSONALITY

A number of personality measures have been devised for the purpose of testing specific theoretical hypotheses. Although these tests are not standardized, validated, etc., they are frequently related to psychological theory of various levels of systematization and sophistication. Information regarding such tests may not only be of value for those seeking instruments for similar experimental purposes but it may well be that some of these instruments, more logically related to theory, will eventually supplant some of the instruments in common use but of limited value because of the hodgepodge of theoretical assumptions which were implicit in their construction.

The concept of rigidity is a frequently studied personality variable which has been related to a variety of clinical and social problems. Cowen & Thompson (23) used the water jar Einstellung problems developed by Luchins as a measure of rigidity and found it related to over-all adjustment as measured by the Rorschach test although it was not related to the Bell

or California Test of Personality. Groups differentiated on the basis of the rigidity test were also differentiated on several Rorschach measures of rigidity. Oliver & Ferguson (72) have made a factor analysis of a number of tests of rigidity. Their results may help clarify some of the differences in various tests of rigidity. Their factors, however, show fairly low loadings and appear to be peculiarly influenced by the choice of tests factorized. Results, too, are of limited value because of the need to make guesses about the meaning or significance of the measures used.

Watson & Mensh (104) describe a methodological design and battery of tests for the evaluation of psychotherapy. Since they hypothesize rigidity as a central concept in therapeutic change, their battery includes several tests of rigidity including homographs, the Level of Aspiration Board, mirror drawing, tests of perseveration and persistence, word fluency, and sentence completion methods. In a study, the purpose of which was to investigate the formation of stereotypes, Wyatt & Campbell (114) utilized a series of pictures shown at various stages of focus. The method may have promise as a measure of rigidity.

Miller (69) used a level of aspiration technique and found significant differences among clinical groups. His interpretation of the mean goal discrepancy scores, however, failed to consider the variability within groups since a number of previous studies have indicated that a tendency to extreme scores is typical of many maladjusted groups.

Block & Block (13) utilized the autokinesis situation as a measure of tolerance for ambiguity. They found that intolerance for ambiguity was related to ethnocentrism as measured by the Berkeley Scale of Ethnocentrism.

Gellerman (36) reports a test of repressed oedipal hostility (GPT) which is designed as a group projective test. The test material consists of six TAT pictures projected on a screen. The subject selects one of four multiple-choice alternatives regarding the outcome. Significant correlations between scores on this test and a variety of political attitude tests (although not in the direction predicted by psychoanalysis) suggest the test may have useful predictive value in individual diagnosis. Grace (41) has also reported preliminary data on a paper-pencil test of hostility. This test is a multiple-choice test of the type where the subject selects what he would do in various contingencies.

Hartley & Schwartz (46) have described a test situation using families of dolls with pictorial backgrounds for the study of intergroup attitudes. Although no data regarding the predictive value of the method are described, the report is provocative. A technique similar to this may theoretically provide a sounder approach to children's attitudes toward their families than some in current use which depend upon far-fetched symbolism.

Cass (17) devised a check list questionnaire which could be given to parents and their adolescent children in a study of parent-child relationships. Parents were asked to indicate not only their own attitudes but also what

they thought their children's attitudes to be. The variables she used were concerned with correspondence between parent and child, and knowledge of goals, feelings, etc. She obtained an awareness identification, and projection score. These variables were found to relate to objective criteria of social maladjustment of the children. Refinement of this type of instrument may lead to valuable clinical as well as experimental applications.

Bugental & Zelen (15) published a preliminary report on a test of self-perception specifically related to self-concept theory of personality.

The absence of tests of humor in the field of personality measurement has been as painful to the reviewer as the absence of humor may be in personal relationships. Construction of a test of a sense of humor, however, is a difficult undertaking. Redlich, Levine & Sohler (79) have described one such test, of narrow scope, which perhaps will lead to others. Their instrument consists of a series of 36 cartoons representing different objects of aggression and distortions which subjects sort into like, indifferent, and dislike categories.

ANALYSIS OF TRENDS

Some six trends appear to the reviewer to be present in the foregoing material.

(a) In general, when so-called global tests of personality are put to some specific predictive test, they fare much more poorly than tests devised for specific purposes even if these tests have little but face validity to speak for them.

(b) When psychiatric diagnostic entities are used either as criterion or as predictors, experimental results tend to lack prediction unless the purpose of the study is for gross screening.

(c) Several studies have been concerned with using the test data to determine the subject's approach to the situation or generalized set, in contrast to attempts to utilize specific responses as indicators of traits. This trend is apparent both in the analysis of projective and objective tests.

(d) In general, prediction for all kinds of instruments is poor when put to careful experimental test with other information regarding the subjects eliminated from the test judgments. Although many studies show correlations significantly different from zero and significant differences in means, findings indicating that a particular test might be relied on for individual clinical prediction are exceedingly rare.

(e) The many negative studies of the past few years are showing their effects in a variety of ways. There has been less inclination to rush into print with new, unvalidated tests. Experimental methodology is distinctly more sophisticated than in previous years; and the principles of cross-validation, elimination of bias, and use of controls are fairly well accepted. There have also been a number of studies specifically concerned with defining the limitations of instruments by showing the importance of examiner and situational variables in test results.

(f) Research in personality theory is producing a number of tests which may be the forerunners of a newer type clinical instrument in which the test is developed as an operation for the measurement of some specific theoretical construct and is related to some more general system. Examples of this are the methods being developed for measuring concepts such as rigidity, hostility, tolerance for ambiguity, level of aspiration, and humor.

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CLINICAL METHODS: PSYCHOTHERAPY¹

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It appears that most that is really new in the field of psychotherapy has occurred in connection with group psychotherapy and with the treatment of schizophrenia. The literature on client-centered therapy, psychoanalytic brief psychotherapy, and psychoanalysis contains a few points that are worthy of comment. There are undoubtedly numerous practicing psychotherapists who do not wish to be identified by any of these labels, but they do not seem to write very much in the scientific journals; sometimes their work finds a place in research reports. In the field of research on psychotherapy, there are signs of considerable progress during the year just ended, though it is not as great as one might have hoped and expected.

GROUP THERAPY

Since, as it appears, everything that is done with individuals by workers of different theoretical points of view is being attempted in groups, in addition to much that cannot be done in individual therapy, the range of topics for papers is very wide indeed. All the well-known features of the individual psychotherapeutic situation—transference, resistance, interpretation, working through, and the like—become topics for discussion in connection with groups. In addition, there are the phenomena arising out of the therapist's technical actions and the patient's behavior that are, or are presumed to be, peculiar to group therapy sessions, the phenomena which for better or worse have led, according to Slavson, to the coinage of over 120 new terms (57). And then there are discussions of various technical problems: the size of the therapeutic group, the advantages and disadvantages of single sex or dual sex groups that are homogeneous or heterogeneous with respect to diagnosis, and the applications of group therapy to various kinds of people—psychotics, criminals, alcoholics, children, and so on. Another reason, perhaps, for the wealth of literature, as well as for the impression of great flux in the field, is the relative openness of the group therapy sessions. The presence of professional observers in the sessions seems to be the rule, and recordings of sessions are common. This certainly favors or perhaps necessitates free and open discussion about procedure and theory.

The work in group therapy at the Tavistock Clinic in London may for several reasons be considered in some detail. It is perhaps not so well known in the United States as are other types of group therapy. The writer, having

¹ The survey of the literature to which this review pertains was concluded in May, 1952.

² The writer is much indebted to Elizabeth Bott for her help in the reviewing of references.

had the opportunity to observe sessions of the therapeutic groups in this clinic over a period of some months, feels more confident of his ability to describe what goes on in these groups than is the case with groups about which he has merely read. Again, since the group therapy at Tavistock is more "psychoanalytic" than any other, comes closer to being, or perhaps is, the psychoanalysis of individuals in groups, it may serve as a kind of standard, or extreme on a continuum, with which other group therapies may be compared. Finally, this form of group therapy, which has evolved during the past five years, is a new and highly challenging proceeding; in this writer's opinion it is destined soon to become in the United States, as it is now in Great Britain, highly influential, as well as controversial.

Sutherland (59) and Ezriel (19) have offered further reports on their work during the year just ended. These psychoanalysts consider their work to be "a strictly psychoanalytic approach to group therapy." The aim, according to Sutherland (59), is "to achieve something comparable to the aims of individual analysis, namely, to make conscious some of the unconscious conflicts in the individual's relations with others."

The therapeutic practice is guided by a clearly formulated theory which owes much to Bion's (7) work in group dynamics, to Lewin's field theory, and to certain ideas of Melanie Klein as well as to classical psychoanalysis. Noting the tendency in the history of psychoanalysis to move away from interpretations in terms of the past and in the direction of transference interpretations, and faced with the fact that groups have no common infantile histories, Ezriel took what was for him "the logical next step,"

to extend the transference hypothesis, and to treat *all* material as "transference material" and hence to use it for here and now interpretations. This means that *everything* the patient says or does during a session . . . is considered as the idiom used by the patient to give expression to his need in that session for a specific relationship with his therapist . . . when listening to patients' discussions I always put to myself the question—"what makes these patients say and do these things in front of me at this moment?"—and I make interpretative comments as soon as I think that I can distinguish in the material three kinds of object relations; one, an object relationship which they try to establish within the group, and in particular with myself and which I would like to call the "required" relationship; another, which they feel they have to avoid in external reality however much they may desire it; and a third which depicts a calamity which the patient seems convinced would follow inevitably if he allowed himself to give in to his secret desire of entering into the "avoided" object relationship (19, p. 120).

The writer can testify from his experience that Ezriel is as good as his word; indeed, everything is taken as an expression of some aspect of the relationship with the therapist, and interpretations regularly take the form: you express this (required) relationship, e.g., solicitude for other members of the group, because you must avoid this relationship, e.g., hostile jealousy of their supposed better standing with the therapist, because you fear this calamitous result, e.g., total and irrevocable rejection.

A second major hypothesis concerns the "common group tension," Ezriel writes:

In the beginning of each session there is always some probing when some member of the group, who seems to feel a particular urge to speak, broaches one subject or another. Often a remark made by one member is not taken up by anybody, apparently because nobody can fit it into what is unconsciously at the back of his or her mind. If, on the other hand, it can be fitted in . . . it "clicks" with the unconscious fantasy of another member, and then perhaps with that of a third, then gradually the subject catches on and becomes *the* unconsciously determined topic of the group until the next interpretation produces closure of this particular phase of the session. Apparently this is so because some aspect of the subject under discussion represents something relevant to the dominant unconscious phantasy in each member's mind. It is my view that in dealing with this common group tension (of a particular session or part of it) every group member takes up a particular role characteristic of his personality structure because of the particular unconscious phantasy group relations which he entertains in his mind, and which he tries to solve through appropriate behavior in the group. It is by analyzing the role which each group member takes up in dealing with the *common* tension in the "drama" performed in that session by the group as a whole that we can demonstrate to each group member his particular defense mechanism in dealing with *his own* dominant unconscious tension, and we do this in the same manner as in individual psychoanalytic sessions. (20, p. 63)

If the reader imagines that it must often be very difficult to discover the "common group tension," he shares the impression of the writer. It would appear, however, that experience and theory are enormous aids. The situation in which the patients find themselves is, after all, essentially the same in group after group so that the therapist in time becomes familiar with certain common themes and the signs by which they are to be recognized. As we know, anyone who works with a fairly comprehensive and closely knit theory, which he invests with some libido, gains facility in remembering and fitting together facts which seem to accord with that theory. In the last analysis, of course, everything depends on the adequacy of the theory.

As suggested above, Ezriel takes the view that "every interpretation, to be effective, must contain a 'because' clause." An interpretation should demonstrate to the patient that he is adopting one course of behavior and avoiding another *because* he fears the supposedly disastrous consequences of the latter. It is at this point that reality testing begins.

Patients are offered, repeatedly, the opportunity to compare their expectations concerning their fantasy objects, which have been projected onto the therapist, with the actual behavior of the therapist.

It is this reality testing which enables patients to give less disguised expression in external reality to the hitherto avoided behavior pattern, and to integrate the experience gained in that session with experiences outside the analytic situation both in their present environment and in their past, especially also their infantile past.

On this same point, Sutherland (59) writes,

In our view, this (reality testing) can only be done adequately in the group by the therapist pointing out to all the unconscious content of the common tension and to each individual his particular defenses against it and his fears of the consequences if this unconscious aim were to be gratified. We do not think that "transference" interpretations to individuals as though there were no group present do justice to the dynamics of the transference situation in a group. (59, p. 113)

It is perhaps necessary to add in order fully to convey the "strictly psychoanalytic" nature of this therapy that the above is just about all the therapist does. What he does not do is also of critical importance for the therapy. He does not do anything to "structure the situation" in the beginning. He does nothing to encourage group members to talk, except interpret the silence or relative silence of individual members. He offers no direct support, nor does he undertake to stimulate significant attitudes. His manner is intended to betray neither permissiveness nor its opposite. He does not mix group therapy with individual therapy with the same patient; nor does he permit communication between an individual group member and himself except in the group. He makes no use of any information about a patient that is not derived from what that patient says or does in the group. In short, he maintains the position of the strictly passive analyst, i.e., the analyst who does nothing at any time except make interpretations. Perhaps the feature of this therapy which stands most in contrast to other psychoanalytically oriented group therapies is its readiness to deal with negative transference; an important guiding notion is that activities, designed to prepare the patient for interpretation or to help things to move along, actually work to prevent the development of negative transference and, hence, may make it impossible for the analysis to go deep.

It may be a good idea to add that when Sutherland and Ezriel say "transference," they almost always mean "unconscious transference." This is the major concept for explaining very diverse manifestations in the therapy sessions, including manifest ("required") attitudes toward the therapist, rather than a term for describing any conscious feelings toward, or overt behavior with respect to, the therapist.

If the various features of the therapy described by Sutherland and by Ezriel be regarded singly, we find that they have appeared in the work of other therapists. The emphasis upon emotional relationships operating at the moment in the group rather than upon the content of discussions, for example, has a key place in the work of Coffey *et al.* (9). The idea of common group tensions, which seems to have originated with Bion, is central in the thinking of numerous group dynamicists who seek to help task oriented groups solve their problems. To place such accent on transference interpretations is, as Ezriel says, a growing tendency in psychoanalysis, though perhaps none has gone so all-out as he. The strict adherence to the role of the analyst as the "screen" is, of course, classical, though it would seem to be far less common today than formerly. It is the particular combination of all these features that is unique—this, and the fact to be gone into in another

place, that the work is sharply oriented toward research, under strong influence by the work of Kurt Lewin.

Group therapy at the Tavistock Clinic is carried out in groups of approximately eight adult neurotic patients. Ordinarily the groups comprise approximately equal numbers of men and women, who are not too dissimilar in age, intelligence or education. There is no discrimination with respect to symptoms; the notion is that patients who would do well in individual psychoanalysis will do well in these groups, particularly if in the initial consultation the relationship of the patient's symptoms to his difficulties in relations with other people are pointed out to him.

Concerning the therapeutic results, Sutherland (59), after pointing to the difficulties of evaluating them, writes

We can say, however, that with a substantial proportion of patients the working through of a set of unconscious fantasies has led to the disappearance of derived symptoms and to changes in character traits just as in individual analysis. . . . [And perhaps more significantly,] most analysts doing group treatment get a strong impression that if the patient can only be given one or two hours of treatment per week, then greater changes are produced by devoting this time to group therapy than to individual sessions.

With this statement the writer is inclined to agree, and when one considers how much individual therapy is performed, on the basis of one session per week, it is easy to see that should this statement become generally accepted the implications would be enormous.

Probably the features of the Tavistock approach that will encounter the strongest objections from American workers are those which spring from M. Klein's (38) theories concerning infantile object relations. These theories not only supply the major content of the preferred interpretations but they provide the rationale for the most striking aspect of the technique, that is, the giving of interpretations, in terms of unconscious transference, as soon as the therapist thinks he sees something to interpret. If, as the theory states, persecutory anxiety resulting from the projection of primary aggression exists in every patient, then it follows reasonably enough that in order for this tendency to come to the fore in the transference, where it can be analyzed, the therapist must maintain a strictly passive role. He must, above all, avoid reassurance or any other activity that would tend to prevent the development of negative transference. But the Kleinian theories and practices have not been popular in this country. It may be expected that before they gain much acceptance as a basis for group therapy, the Tavistock workers will have to answer some rather pointed questions. Is the fact that their brand of group therapy seems to work well in England, where neurotic patients appear to be so wonderfully well-behaved, so unsophisticated psychologically despite high intelligence, a safe indication that it would work as well in the United States, where ego disturbances and "acting out" seem to be so much more common? Is there, as in most presentations of the com-

plex business of group therapy, a gap between stated theory and practice, so that some—enough—subtle directives and discreet reassurances creep into the proceedings? Is it not possible that group members offer ego support one to another, thus making up for the therapist's relative neglect of ego processes and contributing in a crucial way to the therapeutic benefits? Finally, would not progress be about the same if a therapeutic group were conducted precisely in the manner described above, except that infantile object relations were ignored and all communications of the therapist were in terms of, say, the Jungian categories instead?

The answers that we seek will have to come, as everybody knows, from continuing research. What cannot be commended too highly in the Tavistock approach is the fact that research has a central place, that the group therapeutic activities are open for all to see, that means for self-correction are present. It is not at all unlikely that this group therapeutic work will force American psychoanalysts and group psychotherapists to take a fresh look at the "English school of psychoanalysis," something that no amount of clinical reports on individual analyses by Kleinians could accomplish.

In the light of the above, it is not difficult to see why those who practice group therapy at the Tavistock Clinic should regard all other group therapies as less than psychoanalytic, as not quite the real thing. Thus Kelnar (36) of this clinic in his review of Slavson's *Analytic Group Psychotherapy* writes

also it would seem that in spite of his acceptance of psychoanalytic principles, Slavson does not hesitate to dilute them heavily in practice, as when he says: "as treatment proceeds the therapist explains, restrains and criticizes as well as interprets, the patient's behavior and attitudes!

Slavson's work (55, 56) probably resembles more closely the actual practice of the majority of American psychoanalysts than does the group therapy at the Tavistock Clinic. It is certainly different from that described above, and the differences seem to lie chiefly in the fact that it is less rigorous with respect to certain classical features of psychoanalytic technique.

Still, Slavson's work would appear to be closer to psychoanalysis, even to the Tavistock ideal, than is that of most other workers in group psychotherapy. He, like the Tavistock workers, is concerned primarily with the exploratory or uncovering, rather than with the purely supportive, aspects of the therapeutic process; he differs from the Tavistock workers chiefly as regards the techniques by which the uncovering is to be accomplished. Approaches which stand most clearly in opposition to that of Tavistock, and to a lesser extent, that of Slavson, are those which pay less attention to exploration but seek especially to capitalize upon the opportunities offered by the group and its leader for social learning in a favorable, real-life situation. (Approaches which are plainly educational or inspirational offer, of course, the best examples here, but since these are not psychotherapeutic in any strict sense of the word they are left out of account.) This is not a matter of whether or not the approach is guided by psychoanalytic concepts

and theories, but rather a matter of the way in which aims are defined and of the degree of adherence to the classical techniques of psychoanalysis.

Foulkes (24, 25), a Freudian psychoanalyst whose principles and methods have considerable standing in England, is at pains to point out that his work with groups is not an application of psychoanalysis because psychoanalysis involves two people only and cannot take place in a group. Indeed, he considers that it is

of particular importance that the observer avoid the fallacy of transferring the concepts gained from the psychology of the individual, in particular psychoanalytic concepts, too readily to the new field of observation.

Foulkes tries to bring about increased insight in individuals, but he is much more concerned with the constructive or integrative experience which may be had in the group than with making the unconscious conscious. He sees the group as providing, chiefly two significant experiences: "One is adjustment to fellow beings (social adjustment), and to present-day reality. . . . The other is the correction of dependence upon authority." The latter experience is gained by the leader's first allowing the group to place him on an "authoritarian pedestal," and then conducting himself in such a way that they will bring him down so that he is changed from "a leader of the group to a leader *in* the group," who stands for tolerance, reason, understanding, open-mindedness, independent thinking, and so on.

Thus, on a dimension which extends from the strictly psychoanalytic to the strictly educational or inspirational, Foulkes would undoubtedly place himself closer to the latter than to the former extreme. Rainy (47) suggested last year that group therapists seemed about to polarize themselves about the extremes of this dimension. The present reviewer believes he is able to report that this polarization has not yet occurred; indeed, it is his impression that an opposite tendency may now be detected. It appears that relatively few group therapists are content to limit themselves as much as Foulkes does to the benefits of the changing real-life situation in the group. Gordon (33), it must be admitted, comes very close to it; the processes and circumstances of change that he describes, with clarity and convincingness, bear a very close resemblance to those described by Foulkes, but as a client-centered therapist Gordon would probably object to having his method regarded as the extreme opposite of what has been described as the passive technique par excellence, and he might well argue that the changes within individuals, such as the greater acceptance of the self, bear comparison with what the psychoanalysts mean by making the unconscious conscious. Klapman (37) offers a stirring defense of a didactic or authoritative (he says "authoritarian") method with certain types of patients, and yet he writes:

The group therapist who is keenly attuned to the needs and capacities of his charges is most apt to gravitate to the level of a more or less precise admixture (of the analytic and the didactic), for optimal results,

and he, in a very telling way, quotes Freud to support his point. The tech-

nique of psychodrama seems to place heavy emphasis upon the activity of the therapist and the social learning of the patients, yet those who practice it, for example Corsini (11), frequently seem more than willing to interpret to the patient along Freudian lines the manifestations elicited by the role-taking.

Close to a middle position on the present dimension would seem to belong those psychoanalytically trained workers who draw a sharp distinction between psychoanalysis proper and psychoanalytic psychotherapy, and who deliberately apply features of the latter technique in their work with groups. These workers undertake to define on the basis of theory and diagnostic study certain limited aims; they may attempt to remove repression in some areas but not in others; they may consider that their efforts are directed to bringing about changes on some, but not all, levels of the personality; they may offer support or stimulation, assume varying roles, be active in other ways but always, as they hope, with some plan in mind; in short, there is the aim to make the unconscious conscious but with due attention to what the traffic will bear. The work of Frank & Ascher (27), and of Coffey *et al.* (9), for example, would seem to be of this general type. Here, too, in a middle position, would belong the approaches of many workers who employ other theories than psychoanalytic ones, but who seem to include among the therapeutic processes both something on the side of increasing insight, changing the conception of the self, and so forth, and something on the side of learning new social techniques, gaining a better appreciation of other people, removing preconceptions about authority, and the like.

When it comes to the treatment of psychotic patients the situation is, of course, different. Here it seems fairly generally agreed that activity of some kind is the thing. One gets the impression that some kind of group work with psychotic patients has become routine in most mental hospitals, various kinds of technique being used to "reach" the patients—to stimulate, motivate, socialize them. Sometimes the proceeding is justified chiefly on the grounds of economy, but usually some special benefits of the group situation are pointed to.

Concerning the results of group therapy, claims resemble very closely those that have always been made for individual therapy. There is perhaps somewhat more relaxation about it because the investment in the particular patient is not as great, aims are frequently more limited, and there is always the possibility of individual treatment for those who do not make enough progress. The literature contains very few follow-up studies of any sort. The reports of Gordon (33) and of Coffey *et al.* (9) are valuable because of the material they contain concerning patients' accounts of "what it was like." The paper by Peters & Jones (44), which reports improvement in performance by male schizophrenics, on the Porteus maze and mirror-tracing tests, after group treatment and as compared with a control group, is typical of the very few studies that have employed objective tests.

Those who are seriously engaged with research on group therapy, with a

view to discovering the therapeutic effects of particular actions or agencies, are for the most part (as seems quite proper) still at the stage of experimenting with variations in technique, trying to describe and to understand just what happens, formulating general hypotheses and working out methods by which the hypotheses can be tested. Frank (26), Coffey, *et al.* (9), as well as Ezriel (19) and Sutherland (59), have reported efforts of this kind. All these workers and others have stressed the peculiar advantages for research of the group situation, the chief of these being, of course, the fact that the processes at work may be observed by other people besides the therapist. Phillipson (45), approaching the matter from a somewhat different point of view, has suggested that because group therapy sessions may be observed and because they provide a situation in which patients project unconscious fantasies in their observable relationships with the therapist and other group members, these sessions offer the ideal means for validating such projection procedures as the Thematic Apperception Test.

PSYCHOTHERAPY OF SCHIZOPHRENIA

The year just past has seen a significant number of publications in this field, some of which seem to be highly important. The kinds of psychotherapy that have been reported upon, and always with encouraging results, range all the way from casework that is deliberately supportive only, e.g., Pollak (46), to a brand of strict and thoroughgoing psychoanalysis, e.g., Rosenfeld (52).

In what is surely an important paper, Eissler (17) makes a clear distinction between the acute and the "mute" phases of the disease. In the first phase "the patient's ego surrenders to symptoms and accepts them as valid" while in the second phase "the ego is no longer completely filled out by the schizophrenic symptomatology." He then goes on to point out that various therapeutic techniques, even apparently contrasting ones such as Rosen's (50, 51) directive use of deep interpretations and Sechehay's (54) consistently furnishing proofs of maternal love, may very well be effective in bringing patients out of the acute phase. His own preferred approach is one which seeks to communicate with the patient by means of the "primary process."

The more of affect, emotion or passion there is in a psychic process, the closer it is to the primary process. The more it is the product of rational, logical, correct thinking the closer it is to the secondary process,

and the most important thing to be so communicated is the therapist's sincere devotion to the task of helping the patient to recover. Thus, when the concern is with the acute phase of schizophrenia, Eissler aligns himself with Sechehay and with the approach of "unmitigated understanding and acceptance" adopted in the early work of Fromm-Reichmann (28). He goes on to make the significant point that what the very diverse therapies that have been effective with acute schizophrenia have in common is the feature of communication via the primary processes. This may be present in psycho-

drama, group therapy, direct psychoanalysis ("the interpretations which e.g., Rosen gave to his patients would have been of no avail if enunciated in a monotonous voice") and a great many other therapies. It is likely to be present whenever the therapist approaches his patient with excitement, eagerness, and an attitude of great hope and expectation, however much these tendencies may be suppressed or subdued.

Eissler then goes on to state that emergence from the acute phase has nothing to do with the cure of schizophrenia, if by cure is meant a basic change for the better in the structure of the personality. Such a cure, in his view, can be obtained only by means of long and intensive therapy while the patient is in the mute phase. Here Eissler favors a form of psychoanalysis that is significantly different from that used in the treatment of the neuroses. He does not describe the technique in any detail; apparently he believes that various types of intervention are required in order to prevent the patient from returning to the acute phase.

Fromm-Reichmann (29) agrees with Eissler's distinction between the acute and the mute phases of the disease, and with the proposition that emergence from the former phase has nothing to do with a cure. She does not believe, however, that the method of treatment should be different for the two phases or that the psychoanalysis of schizophrenics is essentially different from the psychoanalysis of neurotics. What is essential in all these instances is "the psychoanalytic scrutiny of the defenses against anxiety in the patient's relationship to the psychoanalyst." A further report on the work with schizophrenics at Chestnut Lodge is offered by Szalita-Pemow (60). She not only describes the psychoanalysis of ego processes, the concentration on the ego and its dissociated parts, helping to link them together, but emphasizes the importance of positive work with the patient's superego, to encourage a fusion of the therapist's and the patient's superegos and so to provide more adequate defenses and aid social recovery.

There is no doubt, however, that it is Wexler (66) who has gone further than anyone else in the direction of deliberate approach to the patient's superego. He has described a technique by which the therapist allies himself with the schizophrenic's archaic superego, speaks to the patient in the language of the superego, e.g., agrees with aggressive moral self-accusations, and so gains contact with him. "It is essentially a way of participating in and identifying with the schizophrenic's reality." After contact had been established in the case described, the therapist continued to act as a controlling but friendly superego and apparently the patient in time was able to take over this "good" superego in place of the archaic one, and thus to achieve adequate control and reality sense.

Wexler is careful to point out that this was not the only therapeutic device employed by him in his work with this patient. He notes that signs of improvement regularly followed instances of his assuming the superego role, but he does not wish to suggest that this device would have been effective in isolation from various other aspects of the treatment, or that generalizations

may be drawn concerning the treatment of all schizophrenics. In fact, in another place (67) Wexler shows himself to be an exponent of flexibility. In his discussion of Eissler's paper he writes

First, . . . there is no special value to be attached to our maternalism and no special fear to be attached to our paternalism. Secondly, . . . these are not either/or alternatives in technique, but . . . both are applicable by the same therapist to the same patient, dependent on dynamic, economic and structural circumstances which must be accurately assessed by the analyst.

Wexler will be fortunate, however, if his name is not inextricably bound up with the idea of alliance of the therapist with the archaic superego and if his technique is not applied mechanically by other psychotherapists. It should be noted that Wexler was concerned only with what Eissler would call the acute phase of the disease; he does not suggest that his method brought a cure in the most thoroughgoing, psychoanalytic sense of the word.

Rosenfeld, a "Kleinian" psychoanalyst of London, represents a point of view quite different from those described above. He writes:

Most American psycho-analytical workers on schizophrenia, for example, Harry Stack Sullivan, Fromm-Reichmann, Federn, Knight, Wexler, Eissler, Rosen, etc. . . . have changed their method of approach so considerably that it can no longer be called psycho-analysis. They all seem agreed that it is futile to regard the psycho-analytical method as useful for acute psychosis. . . . A number of English psycho-analysts, stimulated by Melanie Klein's research on the early stages of infantile development, have been successful in treating acute and chronic schizophrenics by a method which retains the essential features of psycho-analysis. Psycho-analysis in this sense can be defined as a method which comprises interpretation of the positive and negative transference without the use of reassurance or education measures, and the recognition and interpretation of the unconscious material produced by the patient (52, p. 111).

Rosenfeld then describes an incomplete analysis of a schizophrenic which employed no more modification of the classical psychoanalytic technique than did M. Klein's analyses of children. All problems of management were left in the hands of nurses, other doctors, patients, the analyst limiting himself strictly to interpretations of positive and negative transference, in accordance with Kleinian theory, from the beginning of the analysis on. And improvement, in the sense of deep structural changes, is reported.

It would seem to be true, as Rosenfeld says, that most American workers employ methods other than interpretation in order to modify directly the functioning of the superego and of the ego. The "English" approach is clearly distinct, but its exponents are no more backward in reporting improvements. It might be expected that the American workers will object to the allegation that their practice is not psychoanalysis, and they might ask of Rosenfeld, "who, indeed, is the deviationist?" A weakness of Rosenfeld's paper is that he fails to offer a theory to explain why his technique seems to work; though an explanation would no doubt be found in a full exposition of Kleinian theory. Eissler might still argue that it is not the interpretations

that do the therapeutic work, but the fact that the analyst, by putting forward his interpretations so insistently and with such conviction, succeeds in communicating with the patient via the primary process, and Wexler might argue that the consistent putting forward of a system, any system, which from the patient's point of view explains everything, is an excellent way of offering the patient a new superego, albeit a somewhat peculiar one, in place of the archaic superego. On the other hand, it appears that none of the American workers, despite their supposed weakness for deviationism, leave interpretation entirely out of account, and for all we know this may after all be the most essential feature of successful treatment.

It is most encouraging to note that some psychoanalysts have taken up Carl Rogers' challenge to record verbatim their therapeutic interviews. Eissler comes out strongly for the publication of full analytic treatment histories, Fromm-Reichmann reports that electrical recordings of work with schizophrenics are being made at Chestnut Lodge, Wexler made recordings of all the sessions with his patient. Rosen has given demonstrations of his technique before fellow therapists. It is at least a corollary of this that the discussions by and among these writers, though they are replete with differences of opinion, give a strong impression that they are none the less really coming to grips with their problems. Rosenfeld's paper, unfortunately, does not contain a very clear account of what actually happened with his case, but Ezriel (19), of the Tavistock Clinic, whose approach has been influenced very strongly by M. Klein, is recording the psychoanalysis of a schizophrenic patient, and thus we should soon be in a position to compare the contrasting English and American approaches.

CLIENT-CENTERED THERAPY

There is a paper by Bergman (5) which seems to fit well into the pattern of researches published by the nondirectivists during the past five or six years. Recordings of counseling interviews were analyzed into categories of counselor and of client behavior with a view to determining which of the latter followed which of the former. It was found that "reflection of feeling" was the only technique that led to insight or continued exploration more often than might be expected by chance. "Interpretation and structuring lead to abandoning of self-exploration." This paper has been criticized, rather severely, by Ellis (15):

obviously, a client who has a) selected a client-centered counselor, b) had a few sessions, and c) has a therapist trained to avoid interpretations may be set to feel more comfortable with reflections. . . . Bergman avoids the question of whether interpretation in general, in any type of therapy, is more effective than reflection.

It is the writer's impression that a considerable number of younger psychotherapists who were brought up in a client-centered setting have been moving in the direction of more thoroughgoing diagnosis of their patients

and adding techniques aimed at personality changes more radical than an altered self-conception. An excellent example of what one might expect to be fairly general is found in a paper by Beier (4). Analysis of counseling records at the Syracuse University Center revealed that, among clients who had to be considered "lost" by the counselors, there was a group who gave up after less than six contacts because the therapeutic situation was too demanding. It is hypothesized that these clients were severely ambivalent toward facing their problems, dimly aware of feelings and attitudes which could not be openly expressed, and hence they felt misunderstood and more anxious when the counselor limited himself to reflecting needs of which the clients were fully aware. Beier proposes that the classical client-centered technique of reflecting "aware-needs" in the expectation that the client, impressed by the nonthreatening quality of the situation, will be encouraged to go deeper, simply does not work in some cases. Here, the therapist will have to go ahead and show understanding of "unaware needs" or needs that are only implied, and then work through by "total contact reflection" and "transference reflection" the heightened dependency feelings thus aroused.

Although techniques change in client-centered therapy, its philosophy seems to remain constant. This is made explicit by Beier, who considers that his modifications are still within the nondirective spirit: it is still assumed, for example, that the primary responsibility for growth lies with the client, the new procedure being designed to establish for the client conditions under which growth is possible. Similarly, we find that Amster (2) in describing a therapy that is supposed to be somewhere between psychoanalysis and client-centered therapy, mentions techniques which would seem to have a place in various psychotherapies, but places special emphasis on the notion of the client as a person with identity and *raison d'être* who has the right to participate in any change in himself and his functioning. The writings of Carl Rogers over the past 10 years have shown modifications in technique with relative constancy in philosophy. This is understandable, for the sources of a philosophy lie pretty deep; as Meadow (40) has pointed out, some of the major features of the client-centered philosophy seem to have roots within the American ethos itself.

PSYCHOANALYTIC PSYCHOTHERAPY

If we include under this heading all those methods of treatment, besides the process of psychoanalysis itself, which have been devised on the basis of psychoanalytic theory and knowledge, then it is clear that a very great deal of such therapy is practiced, and that the amount is steadily increasing.

Psychoanalysts, for example Brierley (8), still show concern about the "dilution" of psychoanalysis by Alexander (1) and other members of the Chicago school, but having insisted on the distinction between derivative, ameliorative activities and psychoanalysis proper, they seem increasingly willing to practice, to recommend, and to experiment with the former. Thus

Colby's (10) book on psychotherapy is presented with the apparent blessing of several senior psychoanalysts, but it is written so simply and clearly as to invite its use not only by psychiatric and psychological interns but by general practitioners. And, as if to underline the "brief" in brief psychotherapy, Saul (53) describes how a woman of 30 who had suffered from severe hypochondriacal ideas for as long as she could remember was, chiefly by means of interpretation, completely freed of the symptoms in two interviews three weeks apart. This is a strong, but probably not unusual, statement by a psychoanalyst to the effect that different cases require radically different treatments of widely varying duration.

The techniques employed by those who practice psychoanalytic psychotherapy are too numerous and varied to admit of summarization here. This follows from what is certainly one of the essential features of such therapy, namely, that it is planned in accordance with a careful diagnostic study of the patient, so that there is some correspondence between the variety in methods used and the complexity of the individual case.

The precepts offered by Stone (58) are perhaps not unrepresentative: the therapist may use any device that seems appropriate in the particular case, he depends heavily upon his initial formulation of the whole case, he makes decisions as to what are the crucial things, leaving useful defenses and avoiding the stirring up of superfluous conflicts; he guides the interviews by questions, having the patient face him in order to avoid tension; he does not stimulate transference or take roles; he sticks to the events and relationships of daily life and the role of the illness within them, he works with whatever favors spontaneous cure or independence of treatment; interpretations seek to relate trends in the personality to immediate realities and to make syntheses of the several levels of the personality.

Such therapy is, of course, admittedly supportive and re-educative in its largest part, and there is apparently a growing concern with how much may depend upon the real characteristics of the person who undertakes to do the supporting and re-educating. As noted below, this concern has been most pronounced among therapists who work with psychotics, but Eisenstein (16) places heavy emphasis on the capacity for "a strong human and supportive attitude" in the therapy of borderline states, on the assumption that the ego is severely involved, while de Forest (12) seems to take the position that what is good for psychotics and borderline states might well be good for everybody. This is reminiscent of Fairbairn's (22) notion that the psychoanalyst must be a "good man" in some objective sense of the term, if he is to serve the necessary function of replacing the "bad object" of the disturbed individual. It also calls to mind Mowrer's (43) concern, in his paper on training, with the personality development of the psychotherapist.

PSYCHOANALYSIS

So many different things are called "psychoanalytic" that it is perhaps

well to have Stone (58) remind us that psychoanalysis is still the name for a distinctive technique. He considers that it is important, for reasons scientific and practical, to be clear about the distinction between psychoanalysis and brief psychoanalytic psychotherapy. It is not enough, he urges, to say that psychoanalysis recognizes resistance and transference; psychoanalysis has other technical precepts which, besides the "basic rule" include the exclusive reliance upon free association, regularity of time, frequency and duration, three to five hours per week, use of the recumbent position, the confinement of the analyst's activities to interpretation, the maintenance by the analyst of an attitude of emotional passivity and neutrality in accordance with which he offers no gratification of the patient's transference wishes, abstention by the analyst from giving advice or participating in the daily life of the patient, absence of immediate emphasis upon the curing of symptoms. It is Stone's belief that changes in any one of these features of psychoanalysis might well affect the dynamics of the transferences, and hence the whole course of a treatment.

It is important to note, before discussing the issues within psychoanalysis that have attracted special attention during the past year, that the above conception of psychoanalysis is undoubtedly the prevailing one among practicing psychoanalysts. And it is well to recall that the technique of psychoanalysis was designed for the treatment of neurotics. Probably most therapists who use psychoanalytic procedures with other types of mental disturbance do so without any feeling that they are helping to undermine the technique of psychoanalysis itself. Similarly, most psychoanalysts who upon occasion perform brief psychotherapy do so with the thought that they have merely adopted an alternative, psychoanalytically guided technique as required by circumstances.

Concerning the technique of psychoanalysis itself, there is undoubtedly a trend, as Raimy (47) noted last year, in the direction of greater emphasis upon ego psychology. Although important papers are fewer than last year, there are enough to indicate a continuing emphasis; for example, Gerö (31) distinguishes between "deep, unconscious automatic defense mechanisms and those located in what may be called the layers of defense near the ego" and gives special attention to the latter, while Meerloo & Coleman (41) in a paper on the transference function argue that "normal transference," which operates in analysis as well as outside, promotes a wide variety of constructive ego functions. There is no doubt that activity, that is, activity other than the giving of interpretations, is common among psychoanalysts who do not regard themselves as in any sense "deviationists." Such activity is usually justified on the ground that it paves the way for effective interpretation, and so partakes of the spirit of classical psychoanalysis. Such activity could hardly be called a new departure in psychoanalysis, since most of its exponents have little difficulty in finding precedents in the work of Freud and Ferenczi, and it is difficult to know whether the aim is to provide supplements

to psychoanalysis that will be useful in all cases or whether the activity is seen as called for by the fact that, in America, increasing numbers of patients with psychotic or psychopathic traits are taken into analysis. At any rate, here is an issue over which psychoanalysts divide sharply. For example, the types of activity advocated by Frumkes (30)—setting a date for the end of analysis, requiring the patient to speak emotionally, forcing fantasies, intervention in the patient's outside life and so forth—would convict him in the minds of English psychoanalysts, of the school headed by M. Klein, of having given up the psychoanalytic technique altogether.

The Kleinians, of course, are not only distinguished for their objections to therapeutic activity rationalized on the basis of ego psychology; they have, as suggested in the above discussion of group therapy, a comprehensive body of theory and precept. Their approach differs so widely from orthodox Freudianism that representatives of these two groups in Great Britain have difficulty in communicating with one another. It is an interesting fact that on the British psychoanalytic scene today, the Kleinian group wields by far the greater power. At the same time, there is in Britain a more or less organized group of psychoanalysts who stand somewhere between the Kleinians and the Freudians. This "middle group" is more devoted to scientific methodology and more interested in research than are those on either side of them. Among them is Ezriel (19) who, as noted above, is recording psychoanalytic sessions. There is hope that, through the "middle group," differences among British psychoanalysts may in time be clarified if not considerably reduced. It must be noted, however, that this group is actually much closer to the Kleinian than to the Freudian position. It may well be that also through them the highly original and enormously significant hypotheses of M. Klein will receive a fair testing in America as well as in England.

What would appear to be a trend within psychoanalysis is the growing emphasis upon the personality, feelings, and behavior of the psychoanalyst. It would be difficult to estimate to what extent, if any, orthodox psychoanalysts have come under the influence of those numerous writers, neo-Freudian and other, who see psychotherapy as a total configuration of interpersonal relationships. It is clear that as the tendency toward activity on the part of the analyst increases, his importance as a real person also increases and demands special consideration.

There was more than a sprinkling of papers on counter-transference published last year, and this year there is certainly no lack of them. If counter-transference is the name for all the feelings and attitudes aroused in the analyst as he works with a patient, then it is obvious that the topic is a very large one indeed, and it is not surprising that different writers take up quite different aspects. For example, Gitelson (32) sounds the warning that wherever a psychoanalyst determines to do something active it has to be considered that he may be "acting out in the counter-transference," while de Forest (12) takes the view that in psychoanalytic therapy a cherishing

counter-transference, i.e., conscious positive feelings toward the patient, are necessary to success. Apparently, however, these two writers would agree that a maximum awareness of counter-transference is all to the good, and Gitelson makes the further point that it is the analyst's openness to the analysis and integration of his counter-transference that "constitutes the analyst's real contact with the patient and which lets the patient feel that he is not alone." For the most vivid accounts of the emotional position of the psychoanalyst, and the truest appreciation of the importance of his real personality one has to turn to the reports of those analysts, as noted above, who have undertaken intensive therapy with psychotics and borderline character problems.

RESEARCH ON PSYCHOTHERAPY

The rather numerous discussions on methodology in this field, published during the year just ended, show not only concern about research design, but increasing understanding of what actually happens in psychotherapeutic relationships and adequate awareness of the dangers of premature or rigid quantification. Papers by Miller (42), Watson (62, 63), Watson & Mensh (64), Eysenck (18), and Edwards & Cronbach (14) are all concerned with the general problem of how to assess the effects of psychotherapy. All are agreed about the necessity for considering variables in the patient, in the therapist, in the therapeutic situation, and in the environment outside of the therapy; all are concerned to find or to devise methods of establishing controls for each of these kinds of variables, and all are impressed by the failures to achieve such controls in the researches of the past. Most of these writers stress the enormous complexity of the problem; they seek to point out all the pitfalls without being completely discouraging. Thus Edwards & Cronbach (14). These last writers render valuable service by calling attention to the similarities between research on psychotherapy and educational research; the comparison makes it plain that researches on psychotherapy have a long row to hoe but, also, a good chance of learning much from the wide experience of their colleagues in education. These writers point to the meaninglessness of those researches, in education or in psychotherapy, which have neglected the "organismic variables," i.e., the variables in the pupil or patient, and they recommend factorial design, e.g., a procedure in which two methods of therapy and two kinds of patients are studied in all possible combinations. They consider that it is better to study 10 cases, 2 each of 5 subtypes, than to study 50 undifferentiated people. They call for complex treatment of the responses to psychotherapy, rather than the assessment of something like over-all adjustment, and for the refinement of assessment procedures. They show how tender-minded, hypothesis-producing activity and rigorous experimental design must be mutually supplementary in this field, first the one and then the other being the object of concentration. All of the above papers should be read by everyone planning to do research on the effects of psycho-

therapy; the paper by Edwards & Cronbach is worth the attention of every psychologist.

If, as seems not unlikely, there are psychologists who are disposed to neglect some of their p's and q's when undertaking research on psychotherapy, they should read Ellis' critique of systematic theoretical foundations in clinical psychology (15), in which he considers that the hypothetico-deductive approach is to be favored over the empirical one, and Thorne's paper on rules of evidence in the evaluation of the effects of psychotherapy (61), a valuable collection of "rules" that have had important roles in the history of medicine. Such readers should have found elsewhere, however, adequate sources of motivation, inspiration and determination.

Three major areas of research activity may be distinguished: (a) Large scale researches designed to show the effects of psychotherapy in general or of particular therapeutic programs; (b) exploratory researches in which workers, usually with the aid of electrical recording devices, attend closely to the process of therapy itself, trying to define, in accordance with theory, relevant variables and to formulate hypotheses which may be tested by more rigorous methods; (c) researches in which a worker has undertaken to isolate and to measure a few of the variables operating in the therapeutic situation and to establish relationships among them.

Two large scale researches on the effects of psychotherapy, which apparently have been in progress for several years, have been the objects of preliminary reports during the year: one is at Washington University [Watson *et al.* (63, 64, 65)], the other at the University of Chicago [Rogers *et al.* (34, 35, 48)]. Both of these projects were launched with the fullest awareness of the defects of previous work; both go to great lengths, and undoubtedly great expense, to establish necessary controls. In the Washington University project all patients entering the hospital are assigned in the ratio of three to two either to an experimental group or to a control group. The experimental group are tested and studied in detail, given psychotherapy (which is recorded), tested at the termination of treatment and again after 18 months. The control group are tested at the time of intake, then, after a period of time equal to the duration of treatment for the experimental group, they are taken into treatment, after which they follow the same program as the experimental group. In addition, there is a group of normal controls. In the Chicago study clients are examined before and after therapy and in follow-up; effects of psychotherapy are appraised by (a) the "own control group method," according to which a group of clients asking for therapy are tested, asked to wait two months before being tested again, then given therapy and tested on the same schedule as the experimental group, and by (b) the "equivalent control group method" according to which comparable subjects not seeking therapy are tested at the same intervals as above. An "attrition group," comprising cases that failed to complete six or more therapeutic interviews, is studied separately. Here, as in the Washington Univer-

sity study, recordings of the therapeutic interviews are made. Thus both of the studies are designed to yield data bearing upon most of the kinds of variables operating in the total therapeutic situation.

What one wonders about in the case of the Washington University study is the nature of the psychotherapy itself. We are told that the psychotherapists represent different points of view—eclectic, psychoanalytic, psychological, active, relationship, and others—and, apparently, the psychotherapy is in some sense "brief." One might hope that it is not so brief as to make the enormous amount of testing seem out of proportion. We are told by Watson, Mensh & Gildea (65) that the therapy is "dynamic," and that there are certain basic factors, implicit or explicit, that are common to all forms of psychotherapy. This last is a reasonable hypothesis. But it is a view that might have been favored by the decision to work with therapists of various theoretical positions, and it is somewhat suggestive of superficiality, or at least of a tendency to treat psychotherapy as an undifferentiated whole.

It could hardly be said of the Chicago study that it suffers from too much eclecticism. The trouble here, of course, is that there is no way of showing that changes in the experimental group as compared with the control groups might not have been produced more readily, along with various other kinds of more beneficial changes, by any one of a variety of other methods. But one research group cannot do everything at once, and a demonstration that certain changes can be produced by one method is not to be regarded lightly. There is no doubt that we stand to learn much from these two projects.

It is important, about these projects, that although the workers are in a good position to turn up information about the precise effects of particular activities within the therapeutic situation, the major concern is with the over-all effects of psychotherapy as compared with no psychotherapy. It is as if the major aim of the researches were to demonstrate that psychotherapy, or a particular brand of it, after all does "do some good." What seems to have been on the minds of many people has been stated rather bluntly by Eysenck (18), who concludes from his survey of studies on the effects of psychotherapy that "they fail to prove that psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients." He goes on to say that this

should give pause to those who would wish to give an important part in the training of clinical psychologists to a skill the existence and effectiveness of which is still unsupported by any scientifically acceptable evidence.

If psychologists interested in studying psychotherapy are made nervous by such statements as this, that is understandable; and the propagandistic effects of Eysenck's paper in the United Kingdom, where psychiatry is predominantly organic in its orientation, is undoubtedly such as to make some psychologists itch to counteract it; nevertheless, in this writer's opinion, the only wise course with respect to such a challenge is to ignore it. From the

point of view of science, the question "Does psychotherapy do any good?" has little interest because it is virtually meaningless. It is obvious that some people change in some ways under the influence of some kinds of therapeutic activities while other people do not change, or change in different ways, under the same therapeutic activity, and that still other people change in ways similar to the above without any therapeutic activity. The question is, which people, in what circumstances, responding to what therapeutic stimuli.

What has, in the above, been labelled exploratory research might better be called, certainly in some instances, empirical research. The researcher who makes recordings of therapeutic sessions with a view to their careful analysis does not begin without any ideas about what he is going to find. On the contrary, he usually starts with a theoretical framework, embodying numerous postulates and general hypotheses, which he seeks to check against his observations. This has always been true of the work of the client-centered therapists, and we may be reasonably sure that it is true of other workers, such as those at Washington University and at Chestnut Lodge, who have begun to use recordings. In the best case, the researcher, who is also the psychotherapist, predicts on the basis of his theory what will be the effects in a particular case of a particular therapeutic action. This is particularly well exemplified in the recent work of Ezriel (21) and of Wexler (66), who seem to be carrying out some suggestions made by Bernfeld (6) in 1941. Ezriel uses in his studies of individual psychoanalyses the same general approach as that described above in connection with group psychotherapy. Wexler's work on schizophrenia, some aspects of which were described above, was designed and carried out with the fullest attention to the requirements of sound methodology. There is no doubt that success in predicting patients' responses to therapeutic actions supplies very considerable empirical support for the validity of the general postulates upon which the predictions were made. There is no doubt, either, that work of the kind being considered is most likely to be of immediate use to the practicing psychotherapist, who must constantly ask himself, "what would be good to do in these circumstances with this patient," and who has reason to be quite happy if he knows that for certain things he might try certain outcomes are probable. It is important to note, too, that work such as that of Ezriel and Wexler is research on personality as much as it is research on psychotherapy. One might say, indeed, that psychotherapy is here used as an instrument for testing, at an empirical level, general propositions about the functioning of personality; and the data of psychotherapy are still, as they have been from the beginning, a major source of new hypotheses concerning personality functioning. Since, as suggested above, sound knowledge of personality must be the basis for a scientific psychotherapy, any approach which yields such knowledge is more than welcome. Psychotherapy, as a means for studying personality, has the very special advantage of being one of the very few situations in which all, or most, of the variables having a place in important theories are made

explicit. There is still, of course, a pretty far cry from the kind of predictions that psychotherapists make to predictions for controlled situations, the kind that finally yield demonstrable truth. However, the experimenter who is looking for significant hypotheses to test would do well, it seems, to choose some of those which stand up well in empirical studies of psychotherapy.

A quantitative study of the process of therapy, which shows awareness of the issues discussed here, has been performed by Dittman (13). This work goes a long way toward overcoming some of the defects of earlier attempts at analysis of therapeutic interview records. The question was "What are the specific activities of the psychotherapist which are most immediately useful to the patient in developing awareness of his interpersonal relations?" Judges rated, on the basis of the first 30 of a series of weekly recorded interviews, (a) the patient's level of awareness of "feelings" referred to by the therapist in his verbalizations; (b) the patient's level of awareness of his interpersonal behavior in relation to the therapist, which the latter referred to in his communications; (c) the therapist's level of participation in the current communications of the patient; and, quite independently, (d) the general development of the patient's awareness of his interpersonal relations with respect to several broad areas of subject matter. It was found that the development of awareness of interpersonal relations on the part of the patient was most favored by the therapist's responding at a high level of participation to both the interpersonal relations and the feelings of the patient at the given time. Response to expressed feelings alone was not sufficient. There was some evidence that progress in the patient was promoted by the therapist's responding to feelings of which the patient was not fully aware. The virtues of this research seem to be that it is well grounded in theory, takes adequate account of the complexity of the psychotherapeutic interaction, and shows that meaningful categories, broad enough to be dynamically significant, can be reliably estimated, and employed in a sound research design.

Almost any research that is directed to the rather painful matter of failure in psychotherapy deserves mention. The empirical study of Koren, Goetzel & Evans (39) is a valuable addition to the literature on this subject. Fifty-four of 268 patients treated at a mental health clinic were classified as failures. Twenty-seven of the failures had fewer than four hours of treatment. Three major causes of failure were discerned: (a) inadequate or ulterior motivation was not dealt with, (b) diagnosis failed to take account of relevant psychodynamic processes, and (c) transference was not dealt with or counter-transference was not recognized. It may be anticipated that these workers will both improve their capacity to predict outcomes of psychotherapy and reduce the number of their failures in practice.

A quite different approach to research on psychotherapy is that which addresses itself to a few variables with a view to their isolation and measurement. Methodological problems are seen as not very different from those ordinarily encountered in the study of social behavior; usually, in fact, it

is considered that findings concerning the therapist-patient relationship might hold also for other types of interpersonal relationships. An interesting example of this approach is Fiedler's (23) quantitative study of certain counter-transference attitudes. He used as subjects 22 psychotherapists, mainly clinical psychologists in a mental hygiene clinic, and their patients, i.e., psychoneurotic war veterans. Patients described themselves by sorting 76 statements supposed to be expressive of one or another of Murray's needs. Each therapist, using the same sorting method as above, (a) predicted how his patients described themselves, (b) described himself as he was, and (c) described himself as he would like to be. Interest focused particularly on the measured tendencies in the therapist to overestimate his patients' similarity to himself, which was taken as an indication of acceptance of, or empathy with, those patients, and to overestimate the patients' similarity to the therapist's ideal self, an indication, as it seemed, of an inclination in the therapist to expect much of the patient, to be demanding toward him. The more "competent" therapists, as rated by their supervisors, tended either to overestimate their patients' similarity to themselves or to be neutral in this regard; none underestimated the similarity. The rank order correlation between rated competence and tendency to overestimate was .59. In the case of "unwarranted assumed similarity" to the ideal self of the therapist, results were not so clear cut, but there was a tendency for the more "competent" therapists to approach accuracy in their estimates, while the less competent ones tended either to overestimate or to underestimate the patient's similarity to their ideal selves.

The rather large amount of data collected by Fiedler was subjected to other modes of analysis and his general approach has been followed up in a paper by Wolfson, Fiedler & Butler (68). The point to be emphasized here is that a way was found to quantify in objective terms certain aspects of the therapist-patient relationship, and that, though there is no claim to completeness with respect to counter-transference phenomena, the results are interesting and probably they can be generalized to other inter-personal relationships.

Barron (3), in introducing his research on predicting the course of psychotherapy, places it in the tradition of those studies that "set out to check up on things that *could* be checked up on in the psychotherapeutic enterprise." This study attempted to predict, on the basis of measured variables operating in six psychiatrists and 33 of their patients, the course of psychotherapy designed to last a few months. The psychiatrists and their patients were given, before psychotherapy began, the Rorschach Test, the Minnesota Multiphasic Personality Inventory, and the California Ethnocentrism Scale. The patients were given a short form of the Wechsler-Bellevue intelligence test, the psychiatrists the Michigan Vocabulary test. It was found that the outcome of psychotherapy, as rated on an improvement scale by psychiatrists other than the therapists, could be predicted with considerable accuracy from

the Minnesota Multiphasic Personality Inventory and the Ethnocentrism scale but that the Rorschach Test was considerably less effective. (This brings to mind the finding by Rogers, Knauss & Hammond (49) that it was impossible to predict from the Rorschach Test, using either statistics on scoring categories or the judgments of three psychologists, whether patients would stay in treatment for more than five interviews.) There were 26 cases with valid records for the Ethnocentrism scale; the scale correctly predicted outcome (improved or unimproved) in 21 of these cases. Intelligence of the patient proved to be significantly related to favorable outcome in therapy. Consideration of the test results of the therapists in conjunction with those of the patients did not improve the efficiency of the tests.

What seems particularly significant, and promising, is the fact that both Fiedler's and Barron's researches were carried forward in accordance with theoretical considerations; both made use of theory concerning the organization of personality and concerning the particular ways in which personality would be expressed in the therapeutic situation. It is not accidental that both of these workers are full of plans for future research. Armed with theory and with knowledge of research design, they succeeded in involving themselves intimately with the basic phenomena of psychotherapy. Barron's activities, in particular, might well serve as one model for the clinical psychologist interested in doing research on psychotherapy. He gave the tests to the psychotherapists and the patients, he interviewed each patient three times—at the beginning of therapy, after three weeks, and at the close of the therapy—he talked with each therapist 10 to 20 times during the course of the proceedings, he read each therapist's running notes, and he held with the therapist in question a formal 2-hr. discussion of each patient at the termination of his or her therapy. It is not surprising, then, that his descriptions of therapist-patient interactions seem convincing and adequate to the material. Barron attempts, with considerable success it appears, to conceptualize these interactions in terms of role-integration and role-perception, arguing that differentiation and flexibility of role structure are conducive to mutually satisfying interactions, in psychotherapy as well as elsewhere. There would appear to be no reason why quantitative indices of variables such as this cannot be devised, no reason why clinical psychologists should not go on applying the precepts of experimental design to larger and more significant aspects of the therapist-patient relationship.

It is suggested from time to time that some kind of moratorium be declared on research in psychotherapy. Perhaps it would be a good thing if clinical psychologists ignored the challenge to demonstrate that psychotherapy "does good," and if they gave up attempts to show that one system of psychotherapy works better in general than another. It could hardly be urged, however, that psychotherapists should give up their attempts to record and to describe in detail precisely what they do and what happens in consequence. Not only does this work promise to produce the best hypotheses for quanti-

tative studies, but it may at once be useful to the practicing psychotherapist. Above all, it would be sad to witness any diminution of those researches which seek to throw light on the functioning of personality in a "two-body" relationship. True enough, this kind of research need not be performed upon actual psychotherapeutic relationships, but there would seem to be no doubt but that clinics and counseling centers offer the best practical opportunities for it.

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COUNSELING: THERAPY AND DIAGNOSIS^{1,2}

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Examination of the professional and scientific literature appearing during the period covered by this review has revealed a disappointing paucity of publications providing information derived from research on counseling. The number of persons professionally engaged in counseling during this period was large; there were 645 members in the Counseling and Guidance Division of the American Psychological Association, and many of these persons were working in colleges and universities in which were located active psychological research centers. For these reasons, and considering the research orientation so strongly stressed by many who write about counseling, one would have hoped that a greater quantity of research would have been reported.

The recent writings of counselors may be classified into two categories: first, those showing signs of acquaintance with and leanings toward psychotherapeutic orientation of counseling theory and techniques; and, secondly, those writings centering major, if not exclusive, attention upon the choosing of occupational goals based upon diagnosis of aptitudes and interests and the selection of appropriate and relevant occupational training to achieve those goals. These disjunctive trends apparently have not progressed sufficiently for many efforts to be made to synthesize them in a new and more inclusive formulation of more generalized counseling techniques and processes.

SYSTEMATIC FORMULATIONS OF CONCEPTS OF COUNSELING

Super's article (75) is an exception to the above characterization in that he attempted to formulate a concept of vocational adjustment in terms of the contemporary concepts of self and self-perception. Super described his attempt at integration in the following terms: exclusive emphasis on diagnostic and occupational facts in vocational and occupational counseling misses a very important aspect of the process; adjustment is a matter of attitudes as well as facts; counseling deals with both emotional and rational content, and more than factual data about aptitudes and characteristics of jobs are needed in counseling, especially facts about the client's attitudes toward self and job; choosing an occupation is, in effect, choosing a means

¹ The survey of the literature to which this review pertains was completed in June, 1952.

² The following abbreviations were used in this review: TAT (Thematic Apperception Test); MMPI (Minnesota Multiphasic Personality Inventory); MVP (Michigan Vocabulary Profile); ACE (American Council on Education); USAFI (United States Armed Forces Institute).

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of implementing one's self concept; vocational guidance is the process of helping a person to develop and accept an integrated and adequate picture of himself and of his role in the world of work, to test this concept against reality, and to convert it into reality with satisfaction for himself and with benefit to society.

Ginzberg's study (35) has implications for bridging the gap between psychotherapeutic counseling and vocational guidance (counseling). He and his associates formulated a concept of the psychology of occupational choice as a result of a cross-sectional and genetic study of occupational choices in white, Anglo-Saxon, Christian males from families with incomes in the \$10,000 to \$12,000 range. The subjects were interviewed at two-year age intervals beginning with the age of 11 years. The open-ended interviews were concerned with the manner in which an individual was currently dealing with his problem of occupational choice. Two additional contrasting groups were used in the study, one of boys from the lower socioeconomic group and another of girls matched with the upper socioeconomic boys used in the main study. In all, apparently 90 subjects were used, eight at each age level.

Three stages are described by Ginzberg in the development of an occupational choice: the fantasy stage, the tentative recognition of problems of choice in terms of future satisfaction, and finally the realistic stage of translation of the tentative choices into the reality of opportunities actually available. The authors conclude that the form of the choice process is similar in different socioeconomic groups, but the content is different. They have described certain personality factors important in the choice process, such as reality testing, development of time perspective, postponement of gratification, and ability to compromise. This suggestive study should be provocative of much thought and further exploration of the psychology of choice.

In an attempt at differentiation rather than integration, Failor (27) suggested certain distinguishing marks of counseling as contrasted with psychotherapy. They are: concentration on normal individuals, breadth rather than depth of relationship, use of a broader range of tools and techniques, greater need for co-operative relationships with agencies of the community, and recognition of the socioeconomic variable as important in vocational guidance.

Other formulations of counseling have appeared in the literature. Jones' revision (50) of an established textbook describes his system of high school guidance in which counseling, as personal interviewing, plays a central but not the exclusive role found in a contrasting system of psychotherapeutic counseling. As was true of earlier editions of this textbook, principal attention is given to ways (program as well as technique) of helping adolescents with problems of educational progress, problems of learning in the class room situation, and problems of vocational choice. Much of Jones' concept of guidance is equated with a broad-gauged program of pupil personnel services. Methods of collecting information about individual students, through case conferences, testing, and in other ways, are discussed and described. A

distinction is made between the guidance function of the classroom teacher and the function of the full-time professionally trained counselor.

Zerfoss (99) has edited a book of source materials in counseling for use by teachers, Y.M.C.A. and Y.W.C.A. secretaries, leaders of Scout and boys' clubs, ministers and church workers, playground, physical education, recreation and camp leaders, industrial personnel workers, and others interested in individualizing their relationships with those whom they serve. Zerfoss centers his attention on counseling individuals with normal problems in a preventive role rather than in a curative or clinical relationship. Short sections are quoted from current and earlier writings of a wide variety of different points of view; thus many aspects of counseling and guidance processes are represented.

Berdie (9) edited a monograph concerned with concepts of counseling and also with programs of counseling services in colleges and universities. In this monograph, Mowrer (60) described an anxiety theory differentiating between counseling and psychotherapy. This position, also presented in earlier writings by the author, assigns to psychotherapy the task of bringing repressed conflicts back to consciousness so that they can be solved on a rational basis. Counseling, on the other hand, is described as a type of service dealing with individuals "suffering from fully conscious conflicts," (60, p. 23) accompanied by normal anxiety. A counselor helps the individual use his rational powers after his repressions have been lifted. Psychotherapy is, therefore, curative, whereas counseling is defined as a person-to-person type of system involving fully rational use of one's thinking capacity to deal with nonrepressed and nonneurotic adjustment problems.

Fletcher (28) described his experience at Ohio State University in the selection and placement of counselors, organizing graduate training programs, and providing in-service training for staff members in counseling. Gilbert (33) described the operating relationships obtaining at the University of Illinois between the counseling staff and other divisions of the University. Special stress is placed by Gilbert on his extreme viewpoint concerning the essential confidentiality of records. No data gathered in the counseling relationship are reported or made available to other departments unless permission is explicitly given by the student, except in certain emergencies. Test data about students are practically never reported to other individual staff members outside of the Counseling Bureau.

Holmes (46) presented a review that stresses trends of counseling developments during the past 20 years. One special trend was noted, namely, the gradual substitution of a psychologically oriented program of counseling for the strictly educational practices of an earlier day.

Various experimental designs involved in the evaluation of counseling are reviewed by Dressel (22). Three types were noted: appraisal of group development, including all members of the group; appraisal of individual development with the attendant difficulty of establishing a control group; and external opinion or appraisal of development by those who do not par-

ticipate in the counseling process. Sources of evaluation material suggested by Dressel are the records of significant remarks and gestures that indicate improvement in problem solving, change in interest patterns, change in attitudes, and change in self-understanding. A second source of evaluative material is the changes revealed in oral or written remarks on the individual's problems and evocative subjects. A third source is the group sessions of individuals with comparable problems and the remarks made in such a context. An argument is made for reconsideration of the rationale of existing tests with respect to their possible use in evaluation.

A systematic and comprehensive, descriptive treatment of clinical methods used in child guidance and student guidance was presented by Watson (82). Topics covered include: diagnostic interview, diagnostic tests, and therapeutic techniques and approaches. Bordin (13) names four uses of tests in counseling: source of information to be given to the client; source of diagnostic-prognostic information for the counselor; stimulus to the client to examine himself; and clarification of the counseling process for the client. Gustad (40) discusses the function of test data with regard to their uses in helping the client to learn important facts about himself.

Wise (95) presented a systematic outline of the counseling functions of Protestant ministers in their assistance to individuals, centering emphasis upon simplified therapeutic relationships.

EVALUATION OF THERAPY

The January (1952) issue of the *Journal of Clinical Psychology* contained a symposium on research design in clinical psychology. Certain articles of this symposium are of interest for this review. Ellis (26) discussed the advantages and limitations of an empirical approach in clinical work not grounded upon a systematic formulation. In contrast, he discussed a hypothetico-deductive approach in which experiments and instruments are derived from a consistent organized theory, a theory operationally grounded, undogmatic, and clearly stated in verifiable terms. Sells (70) discussed some problems of criteria and validity in diagnoses and therapy, especially stressing the importance of testing field situations and individual variations in background and attitudes of subjects; the units of measurement used in validation with especial reference to the problem of interpreting a single score in appraising a personality; and, finally, methods of deriving or selecting criteria. Sells also advocated an empirical approach in validation beginning with the study of individual subjects in a criterion situation in order to identify and measure the factors intrinsic to the criterion with empirical validation at each step. The approach is similar to that used in the case of the Strong Interest Test and the Stanford Binet test of intelligence. Thorne (77) listed a number of cautions against claims of proved success of any particular method of therapy. His dicta include the following: any and all methods, including strictly quack techniques, succeed in some cases; initial success cases may soon remit; time alone may have some curative effect; and

subjective reports of success by patients or clinicians oftentimes are unreliable. He asked if anything could be done about the perplexing problem of sampling, since those clients most susceptible to a particular type of treatment are most likely to go to the corresponding type of therapist. Thorne then specified 11 conditions which must exist before validity of the therapeutic claims can be considered established. These conditions range from the clearer establishment of a genuine pathological condition which the therapist "cures," to the gathering of large scale statistical data in contrast with a few selected case histories. In his judgment, current schools of counseling do not always adhere to these principles in support of their therapeutic claims.

The experimental literature involved in the use of measures before and after therapy was surveyed by Berg (11). In all, 23 references were included in this review, and six types of measures were identified. They include ratings, retests with inventories, physiological and organic measures, environmental and achievement correlates, verbal behavior, and experimentally induced maladjustments in human beings and animals. Berg called especial attention in his summary to the curative effect of the mere passage of time and also to the necessity for controlling the effect of concomitant extratherapeutic influences. Edwards & Cronbach (24) identified four types of psychological research: technique research, survey research, administrative research, and critical research, the last being designed to produce generalized knowledge. The authors concluded that research in therapy has been mainly of the first two types, and discussed at length the topic of factorial design and research in psychotherapy, arguing especially against the practice of grouping all response variables into a single measure of adjustment. Watson (83) discussed the type of variables needed in evaluating psychotherapy, including those intrinsic to the patient, those related currently and historically to the patient's life development, those descriptive of the therapy techniques used, and, finally, those descriptive of the therapist.

Watson & Mensh (84) outlined procedures and techniques to be used in the evaluation of the effects of psychotherapy, including various types of projective techniques and ratings. The authors then applied, by way of illustration, the methods to a single case of a 28-year-old male graduate student patient (85). Watson, Mensh & Gildea (86) outlined a research design for purposes of evaluation of the effects of psychotherapy. Data would be collected from many sources, including subsidiary workers in a hospital study, in this case, in an outpatient clinic of a medical school. The case history data from the many sources would be subjected to factor analysis for the identification of clusters found in the data. Studies were to be made also of the interrelationships of various judgment ratings, the longitudinal and developmental aspects of the clients, and the characteristics of the therapists.

An experiment on the effectiveness of directive versus nondirective methods in vocational counseling involving 46 students who volunteered for counseling subsequent to an announcement of its availability was reported by Carlson & Vandever (17). All subjects were given the odd numbered cards

of the TAT² and then divided into three groups—one given directive therapy “as defined by Williamson,” one given nondirective therapy “as defined by Rogers,” and one control group given no counseling. Finally, all groups were given the even numbered cards of the TAT. As indicated by the differences on the TAT, given pre- and postcounseling, all subjects in the three groups moved in a positive direction with no differential effects identified between the directive and nondirective groups. The authors’ explanation for the “small but consistent” tendency for directive therapy to be rated more favorably by clients is that “the counselor felt more at ease in the directive situation.”

PROJECTIVE TECHNIQUES

In their book on *Projective Psychology*, Abt & Bellak (1) have contributions from 12 authors on the use of eight projective techniques. The book summarizes current clinical practice in psychology, business, industry, and action research. Considerable space is devoted to a discussion of theoretical aspects and foundations of the clinical use of projective techniques. Of special interest is Bellak’s discussion of projection conceived as a theory of apperceptive distortion, with projection located at one end of a continuum and objective perception at the opposite end. Abt identified four significant trends in projective psychology: personality is viewed as a process rather than as a trait aggregate; process is determined by interaction with environment and states of need; field theory provides an appropriate frame of reference in which to order data from projective methods; and two classes of propositions about personality are significant, namely, dynamic (field theory) and genetic or historical development. Reichard (66) argued for the use of such projective techniques as Rorschach, TAT, and Szondi, along with the Wechsler-Bellevue, to provide clues to the therapist in identifying the following dimensions of personality: mental efficiency, passive needs versus independence, insight, hostility, personality integration, transference, and suicidal trends.

Six articles concerned with the relevance of TAT for counseling were noted in the literature. Lindzey (54) wrote on the interpretive assumptions and related empirical evidence concerning this test. The assumptions identified include: the subject reveals his own conflicts, dispositions, and motives in his structuring of an unstructured stimulus; the subject usually identifies with one person in the drama; dispositions of the story teller may be represented indirectly or symbolically rather than directly; some stories reveal much more significant material than others; themes arising directly from the stimulus are generally less valuable than those determined more indirectly; recurrent are more likely than once occurring themes to be accurate representations of impulses and conflicts; stories may reflect enduring dispositions or conflicts and impulses of only momentary occurrence; stories may represent not only the subject’s own past but also his observations; stories may reflect group membership and sociocultural factors as well as individual

traits; dispositions and conflicts inferred from the stories may not be manifest in overt behavior or in consciousness. The author argued for the plausibility (not the validity) of these assumptions and cited some evidence in various published studies and case studies.

Goodman (36) presented an indirect validation of a TAT scoring manual using a ratio S/R scores on the TAT, where S is a composite of TAT features used to predict anxiety and R is the sum of "need" and "press" as scored according to Murray. Using 40 white, male neurotic and borderline patients whose symptoms were noted in each of several interviews and who were rated for anxiety by social worker and psychiatrist, the author concluded that his hypotheses were validated at the .02 level of significance; i.e., patients whose presses and needs are greater than their anxiety on the TAT have less anxiety and less symptom change than those whose anxiety on the TAT is greater. Shneidman (71) reported a study in which 15 clinical psychologists contributed blind interpretations of one subject's TAT and Make a Picture Story Test. Such interpretations are compared with additional test and behavioral data and also are subjected to preliminary statistical analysis. The significance of the study possibly lies in the comparative study of the interpretations of various experts. Brackbill (15) studied the effect of color on a modified TAT consisting of six achromatic pictures and six pictures tinted. The author concluded that the color effect, by stimulating the expression of the subject's mood, may arouse unpleasant or conflicting associations. The possibility of faking TAT responses was studied by Weisskopf & Dreppa (90) who concluded that variations in instructions to the subject did produce statistically significant differences at the .05 level or better in the rating of adjustments in five of the nine areas rated.

Wallen (81) reported a study involving 40 students who rated 12 pictures from the ego-vector of Szondi's test, according to a list of trait names given as paired opposites. Then 28 new subjects gave preferences and dislikes with respect to the 12 pictures. The two independent ratings were in general agreement. The author concluded that the dynamics of choice in Szondi pictures are not related to the disorder of the person pictured. Rather, they seemed to be related to perceived traits such as friendliness, kindness, and sociability, which in turn depended upon facial contraction for their perception by the subject. Szollosi, Lamphiear & Best (76) analyzed the choice reactions of 283 "normal" adults with respect to each picture of the Szondi test. They found wide dispersion within each category and that the pictures in any one subset are not equally rated or liked. The rank ordering of all 48 pictures, regardless of category, showed that the choices with respect to each category spanned almost the entire range of variation. Therefore, the pictures were unequal in stimulus strength and the quality of the stimulus in each of the categories was not the same. The reaction to the individual pictures may be valid reflections of the subject's need system, but classification on an *a priori* basis may result in the loss of important data and meaning.

Deri's contention that change within Szondi's profile is a function of in-

telligence was refuted by Cahill (16). The author used two groups of adolescents of both sexes with a total N of 77 divided into subgroups of known IQ's. Cole (20) computed the test-retest reliability of Szondi profiles on 86 college students and found great variations in response to the second administration of the test and concluded that a single administration is not reliable. Borstelmann & Klopfer (14) reported a study involving 400 students and designed to equate the stimulus value within each set and category of the Szondi test. Using the student's choices and ratings as to affective stimulus value, the authors rearranged the 48 pictures into six sets with relatively homogeneous affective values in each set and with one picture from each type or category in each set. The pictures in the new arrangement were given to two groups totaling 122 additional subjects; the analysis revealed greater homogeneity than the original arrangement both within the set and within the category for frequency of choice. The authors concluded that with this new arrangement, the test is now ready for empirical validation against external criteria.

Anderson & Anderson's general introduction (2) to the use of projective techniques includes contributions by 28 authors and covers such methods as the Rorschach, the Four Picture Test, the TAT, the Rosenzweig, the Bender-Gestalt, the Szondi Test, the Wechsler-Bellevue, the Stanford Binet, and such miscellaneous things as doll play, psychodrama, human figure drawings, children's drawings, finger painting, and graphology.

A number of studies on the Rorschach may add further to our interpretation of the value of this test in therapeutic counseling. Palmer (62) presented a study involving two methods of validation, one a matching method and the other an item by item method of personality description. The author concluded that the check list approach is not applicable to validation, but the matching approach did test essential features of the projective method. Hales (43) attempts to clarify the Rorschach test by developing scoring profiles utilizing T -scores for 28 Rorschach factors based on normative data derived from 518 cases, of which 99 were normal. The author was seeking to develop a coding method of the Rorschach similar to the MMPI² coding. Wittenborn & Mettler (98) reported the development of a Rorschach ratio measuring a lack of perceptual control. Wittenborn & Holzberg (97) reported a validity study of the Rorschach using two criteria: psychiatric diagnosis and a symptom cluster score consisting of ratings on nine different clusters. One hundred ninety-nine mental patients of five diagnostic groups were studied. The authors concluded that descriptive aspects of the behavior of psychotic patients are not identifiable or predicted from the Rorschach scores.

Molish (59) reported significant differences between normals and neurotics and normals and schizophrenics using indices of the P responses in specified areas of the Rorschach plates. Barry, Blyth & Albrecht (6) did not find support for the general hypothesis that changes in adjustment rating would be associated with changes in the Rorschach factors for 31 subjects tested

following therapy. Holzberg & Belmont (47) attempted to find a common psychological rationale relating the subtests of the Wechsler-Bellevue and the Rorschach test. Out of 45 predictions made between Rorschach factors and scores on the subtests of the Wechsler-Bellevue, only four showed statistically significant association at the .05 level. Cass & McReynolds (18) presented norms in terms of means, sigmas, and percentile rank distributions for Klopfer's determinant, location, and content categories on 104 subjects. Fonda (29) compared Rorschach and Guilford-Martin G A M I N test behavior of 150 college students. Contrary to theoretical expectancy, the white-space responses on Rorschach are not related to agreeableness, co-operative-ness, or inferiority feelings as defined by Guilford-Martin. However, a correlation of .895 was found between white-space response and the use of question marks as a response to the Guilford-Martin test, with the total number of Rorschach responses partialled out.

Using 60 male students divided into two equivalent groups matched on age, college level, and a number of the more common Rorschach factors, Eichler (25) determined the effect of anticipated electric shock designed to augment the fear-producing nature of the situation upon a digit-subtraction task. The experimental group made significantly more errors on the task and rated themselves significantly more anxious because of the shock stimulus. The author concluded that failure to obtain significant results for the other commonly used Rorschach indicators of anxiety or stress, in this situation of no-stress versus rather high level of stress, is evidence of invalidity of these factors for measuring this type of stress. In an investigation of color shock in Rorschach records of 30 students and with a control group given Rorschachs with achromatic photographic reproduction of colored cards, Meyer (57) analyzed the results of 12 standard signs of color shock. Twenty-four members of the experimental group and 25 members of the control group showed at least two indices of color shock, and the two groups did not differ significantly on any of the 12 indices used. The author concluded that what has been referred to as color shock must be determined by something other than color since it occurs as much when there is no color as when color is present in the stimulus. Rosen (67) asked 193 students to designate Rorschach cards which seemed most appropriate as a father symbol, mother symbol, male sex symbol, female sex symbol, and 16 other symbolic meanings. Cards four, six, and seven were perceived respectively in some cases as father, male sex, and mother symbols. However, the results also indicated great individual differences in perception of symbolic meaning, no single meaning being assigned to a specific card by a majority of any group. The author concluded that the Rorschach consists of stimuli which have a partial, but not total, symbolic communality for different subjects.

OBJECTIVE PERSONALITY TESTING

Purcell, Drevdahl & Purcell (65) analyzed the subtests of the Wechsler-Bellevue test and concluded that there is a reliable relationship between

anxiety as measured by the MMPI and altitude-IQ discrepancy. Wittenborn & Holzberg (96) and Schofield (69) in separate reports concluded that the Wechsler-Bellevue was of no use in measuring nonintellectual personality factors. Schofield (69) analyzed 34 studies on such diagnostic use of subtests and found that in 4, the results were ambiguous, in 9 they were positive, and in 21 the findings were negative. Hunt & French (48) described a "culture and education free measure of general ability" which will also serve as a screening device for psychopathology. Kirkpatrick (52) analyzed 288 items of Jurgensen's Forced-Choice Classification Index with 179 subjects. His results indicated the need for cross validation of such studies on a new population. The development of a self-confidence key on the Jurgensen inventory was reported by Mais (55). This scale was susceptible to falsification. Barron & Welsh (5) described the development of a test for measuring artistic perception as a possible factor in personality style. Hampton (45) used the MMPI to develop an alcoholic scale of 154 items which differentiated between the scores of 84 alcoholic males and 84 nonalcoholic males.

Several studies were identified in the literature involving the MMPI. Gough (37) developed an anti-Semitism scale and presented correlations with other scales and with other variables, including scholarship. Williamson & Hoyt (93) presented some differentiating characteristics of the scores of student leaders classified as to type of activity. Welsh (91) derived an anxiety index and an internalization ratio. Winfield (94) investigated the relationship between intelligence and the statistical reliability of the MMPI. Cauffiel & Snyder (19) analyzed the distribution of scores of college populations. Tyler (80) presented a factorial analysis of MMPI scales and identified three factors described as general maladjustment, conflict, and social aggression. A fourth factor (social adjustment) is heavily loaded with *Mf* and *St* (Status) and negatively loaded with *Pt*; a fifth factor is loaded with *Mf* (negative) and *Hs*. A study of norms and reliability of the MMPI by Gilliland & Colgin (34) repeated the familiar conclusion regarding the generally elevated nature of students' scores. Guthrie (42) summarized the physical complaints of and clinical impressions on 1,104 patients, classified with regard to type of symptom and related these data to MMPI scores. The symptomatic categories were abdominal distress, upper gastrointestinal distress, depression, fatigue, and exhaustion.

The development of effective items in the measurement of personality integration was investigated by McQuitty (56) who concluded that subjective items are more internally consistent than objective ones and more valid when the "Yes-No" preference bias is controlled. The author contended that subjective items are more vague and, therefore, are testing central rather than peripheral attitudes and are less stimulus-bound in response than are objective items. Bills, Vance & McLean (12) reported an index of adjustment and values involving the measurement of a self concept, the acceptance of self, and the ideal self. Pepinsky, Vanatta & Siegel (63) developed a group participation scale showing considerable spread of distribution of scale values.

Sims (72) reported the use of a ranking of selected occupations as a subtle indication of an individual's social class identifications. Using the Minnesota Personality Scale and the Cornell Index, Noll (61) found successful simulation of scores of adjustment.

Schofield (68) summarized research literature of 1950, contained in 148 studies classified according to validity and normative data on projected techniques.

MEASUREMENT OF INTEREST

The relationship between experience and interest as measured by the Kuder Preference Record was determined by Dressel & Matteson (23). Seventy college students were given the Kuder test, first with the standard instructions, and secondly, with instructions to respond on the basis of experience rather than interest. Correlations between the interest and experience indices ranged from .88 to .53 with outdoor and artistic keys giving the highest relationships and clerical and computational keys giving the lowest relationships. In general, all keys correlated highly on these two indices. In another experiment, 30 additional students checked the Kuder using two different sets of instructions at the same administration of the test. Using the DuMas coefficient of profile similarity, 29 pairs of profiles out of 70 were identical at the .05 level. If this experimental design is sound, then apparently interest and experience are highly correlated.

Steinberg (73) made a study of the relationship between vocational preference and emotional adjustment, using 100 male veterans drawing compensation for nonneurotic disability and an additional 50 cases drawing compensation for neurotic disability. These cases were "selected at random from the files," and the nonneurotic group consisted of those not exhibiting psychosomatic symptoms or complaints. The neurotic group scored significantly higher than the nonneurotic group on the literary and musical scales of the Kuder test. The neurotic group also scored lower than the Kuder base group on the science and mechanical scales and higher on the literary and musical scales. All of these differences were significant at the .05 level. Biserial correlations of the three scales with the dichotomy neurotic-nonneurotic give the following coefficients: mechanical key, $-.31$; literary key, $+.38$; musical key, $+.30$. All coefficients were significant at the .01 level. The author concluded that neurotics choose an occupation with an emphasis on fantasy rather than on practical performance—those requiring a high order of talent for success and a high reward for achieving success—whereas the nonneurotic chooses practical fields, such as mechanical, scientific, and computational work.

Knauff (53) administered the Strong Vocational Interest Test to 38 shop managers in retail bakeries. They received a mean score of "B+" on the production managers' key, and a mean of "A" for office manager. None of the 39 occupational keys nor the three clinical keys correlated with success or failure as indicated by criterion groups of the highest and lowest 27 per cent on a controllable cost ratio index. The author constructed a shop managers'

scoring key of discriminating items between the two criterion groups, this new key correlating .53 with job success with a new criterion group of 32 manager trainees and new managers. Webster, Winn & Oliver (88) reported preliminary findings of an attempt to develop selection tests for engineers. Bennett's experimental form of a test of productive thinking and the Miller Analogies Test were given to 54 employed engineers. The Miller test correlated .06 and the thinking test, .24 with ratings of "promise." In a study of the Strong and Kuder interest tests, administered to 34 salesmen and 52 research workers, all with engineering training, the authors concluded that the Strong test yielded most significant results for their purpose.

To determine the effect of a selection situation upon results of tests of interest and temperament, Green (38) studied two groups of policemen who were applying for transfer and a control group of men working in a juvenile delinquency agency. The author concluded that the Kuder Preference Record and the Guilford S T D C R tests are susceptible to faking, but the Guilford-Martin G A M I N test appeared to be relatively non-fakeable. Webb (87) described the development of a generalized scale for measuring interest in science subjects in school. Both the Likert and the Thurstone techniques of scaling were employed. Six natural science subjects were investigated: botany, chemistry, geology, physics, psychology, and zoology.

Strong (74) reported a comparison of the interest scores of 345 college men with the occupation engaged in 20 years after the original testing. He found a high agreement between interest scores on tests taken in college and the student's present occupation. For those subjects who had not changed their occupation in this 20-year period, there was a 91 per cent agreement between the scores and the occupation. For those who have changed occupations during this period, there was 77 per cent of the maximum possible agreement. Continuous employment in an occupation results in a very slight mean increase in the Strong score for that occupation; that is, from a mean of 44.5 to 46.8. Change in an occupation results in an equally small change in test scores at first and then in greater changes. Those who changed their occupations in the first few years after college graduation had a mean decrease in score for that occupation from 40.6 to 39.6 a few years later. The author concludes that men who change their occupation do not have as high a mean score for that occupation as do men who do not change their occupation following college graduation.

APTITUDE TESTING

Barnette (3) analyzed the records of 890 counseled veterans to determine whether test pattern data in selected test batteries differentiate between successful and unsuccessful members of an occupation, between these groups and estimated population means, among the various occupational areas themselves, and between individual, successful veterans and those classified as less successful or as failures in an occupation. The population studied represented diverse occupations and was designed to determine occupational suc-

cess regardless of type of occupation. The sample used had a mean age of 25.7 years and a mean educational level of 12.6 grade. Fifty-four per cent of the population were in or planning on the managerial or professional types of work. A total job adjustment and progress score was derived from a questionnaire concerning the facts reported by the individual with respect to his occupational success or failure and a criterion score was set. Ninety-eight per cent of those who failed to undertake the recommended training, following counseling, were classified as failure by the criterion, while 96 per cent of those who completed training were classified as successes. The author found that, in the case of engineers, the patterns of tests used clearly differentiated the success and failure groups and also differentiated the engineers from the general population; the sales group pattern showed few differences; the accountancy success and failure groups were better differentiated by the aptitude tests than by the Kuder scales; the general clerical group showed few differences, but specialized clerical groups showed clear differences between success and failure subjects. The author concluded that the concept of occupational aptitude patterns is justified by the findings of his study.

Halliday, Fletcher & Cohen (44) reported a validity study of the Owens-Bennett Mechanical Comprehension Test involving 105 freshman engineering students at Ohio State University. The test correlated .42 with fall quarter grades and .40 with first year average grades. It correlated highest with grades in engineering drawing and in geometry. Those students who were dismissed, withdrew, or transferred had significantly lower average scores than those who remained in college to the end of the year. But the Owens-Bennett test added very little to the correlation obtained by the Ohio State Psychological Test alone. Jones & Smith (51) reported the development of a test of supervisory ability standardized on 72 supervisors. The scale included personality, personal history, and problematic items. Ratings of executive ability correlate to the extent of .40 with the total score for problematic and personality items; .16, with personal history items; and .45, with the total scale. Bass & Stucki (7) reported a modification of the Purdue pegboard allowing a longer time with a larger board and with more pins and washers. The reliability coefficients increased from .60 to .80 and the correlations between the old and the new forms are as high as the reliabilities will permit.

A study of the validity of the Minnesota Occupational Rating Scale for four types of abilities—academic, mechanical, clerical, and artistic—for which corresponding standardized tests are available was made by Geist (31). One hundred and fifty counseling records were used and the 430 occupations in the scale were categorized into the nine broad areas of the *Dictionary of Occupational Titles*. Mean ability scores were calculated for each occupational area. For professional, managerial, sales, and clerical occupations, there was a marked agreement between the empirical and the a priori profiles. Agriculture showed a very similar profile but with the empirical data attaining higher scores than did the ratings. Personal service occupations showed

a higher empirical score than did the scale profile. Skilled and semiskilled profiles showed the empirical scores for art, clerical, and academic ability somewhat higher than those presented by the Minnesota scales. The author concluded that the actual test data agreed fairly well with the ability ratings included in the Minnesota scale. But the author used test data from counseling cases of those subjects who had expressed a desire or objective rather than data from a criterion group of successful individuals in the occupation.

Ghiselli & Brown (32) summarized the literature, beginning in 1919, on validity of aptitude tests for predicting trainability of workers. Aptitude tests were classified under 30 headings and the results of studies were summarized accordingly with the validity coefficients of different tests grouped together. A weighted mean validity coefficient was found by *Z* transformations. The categories were based on "superficial similarity in content." Occupational groupings were reported for 17 types, such as, general clerk, vehicle operator, structural worker, and protective service. The authors concluded that trainability in most occupations may be reasonably well predicted by the selection with tests with a high validity. The median validity coefficient among those tests studied was .42.

Greene (39) reported three validity studies of the MVP.² The highest 66 of 250 editorial workers scored significantly higher than 66 rated lowest on five divisions of the MVP; with 37 engineers sorted into six groups, performance correlated .87 with three scores on the MVP combined; four subtests differentiated the 15 most effective from 10 average men in a managerial consulting firm.

Traxler (79) presented results of testing in the field of accounting using measures of intelligence, knowledge of accounting principles, vocational interests, and personal qualities. An orientation test on knowledge of accounting principles correlated, on the average, .43 with college grades. The orientation test correlated .35 with the ratings of success, whereas the achievement test correlated .50 with these ratings. Michael, Zimmerman & Guilford (58) reported an investigation of the major spatial relation and visualization factors involving the analysis of the test scores of 151 boys and 139 girls ranging in age from 15 to 20 years. Factor analysis yielded results indicating that the two factors of spatial relation and visualization remain distinct. Bennett *et al.* (8) reported for the Differential Aptitude Test new norms for Form A on 36,000 cases with wide geographic distribution, information on the use of the test in three colleges, and some correlations with other tests.

PREDICTING ACADEMIC ACHIEVEMENT

A study in the prediction of grades in a dental school with the grades classified in two categories, theory and technique, was published by Weiss (89). With 305 dental students in three dental classes, he found that pre-dental grades, and also factual information scores from a special science test, correlated about .40 with dental grades. The reading test also correlated

about .40 with theory grades. Intelligence tests correlated from .10 to .30. With technique grades as a criterion, a test of carving skill correlated .34. The visualization test correlated .24 with this criterion.

Barrett (4) found that the *Q* subscore of the ACE² Psychological Examination correlated low with grades in mathematics as did also the *L* subscore and the total score. Berdie, Dressel & Kelso (10) reported a study involving students in 13 universities, and a total of 7,039 cases, in which the range of validity coefficients of the ACE was from .25 to .66, using total scholarship as a criterion. The *Q* score correlated from .15 to .55, and the *L* score from .18 to .65. The *Q* score correlated highest with mathematics and biological sciences, whereas the *L* score tended to correlate higher with English but also high with grades in social and biological sciences. Jepsen (49) reported that for 797 students graduating at Fresno State College between 1929 and 1947 there was no relationship between academic grades and later occupational success.

Various difficulties encountered in securing adequate criteria for validating differential aptitude tests and making differential occupational and training predictions are discussed by Wesman & Bennett (92). Pierson & Jex (64) reported a study involving 216 engineering freshmen in which multiple correlations with grades of .66 and .68 were secured from a combination of mathematics, social science, and natural science scales of the Cooperative General Achievement Test together with high school grades. Traxler (78) reported a multiple correlation with medical grades of .50 using the USAFI² test of general educational development and the Miller Analogies Test. The USAFI test battery correlated .48. Coleman (21) reported a study involving the adjustment of high school grades equated and adjusted among different high schools. Higher correlations of high school grades with college grades were obtained in some cases than those obtained with unadjusted ranks. Gustad (41) reported a study involving the *Q* and *L* score of the ACE as related to the primary patterns of the Strong Interest Test. He concluded that vocational interests are not conditioned by differential aptitudes, so far as this study is concerned. Frederiksen (30) suggested that counseling use of test predictions would be greater if complete studies of all important validity statistics were published in a volume of well-indexed expectancy tables for different universities and colleges with respect to prediction of grades for students of different levels of ability and graduated from different high schools.

EVALUATIVE COMMENTS

An overview of the published studies of this year reveals some gaps and some significant trends. Publication of studies on the processes of counseling were fewer than those appearing in earlier years. A reduction is also evident in attempts at evaluation of outcomes. In contrast to so few research studies, an increase is noted in attempts at systematic formulation of concepts of counseling, in partial aspects or in totality (Watson & Mensh, Ellis, Sells,

etc.). Especially promising and significant for vocational counseling is Super's reformulation of vocational choice in terms of self concepts. More such "re-writing" of counseling in terms of psychological theory are needed to provide a broader base for clinical practice. Parallel to these beginning attempts at systematization is the subjecting of generalizations derived from limited cases in clinical practice to the scrutiny of experimentation design and critical statistical appraisal of large unbiased samplings (Rosen, Meyer, Schofield, etc.).

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SPECIAL DISABILITIES¹

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The term *special disabilities* is, in a sense, an anachronism in modern psychology. It originated in a period in which a more atomistic approach to the study of human beings prevailed; therefore, it is somewhat out of keeping with present-day concepts of global intelligence, total personality, and the whole child. Nevertheless, the term makes a convenient heading for the discussion of certain part dysfunctions, and it need not be forgotten that they occur in organisms that function as wholes, or that their presence may have far-reaching effects upon other aspects of personality. A part dysfunction or special disability, for the purposes of this review, is any defect or disability that may occur in an otherwise normally functioning person. A further requirement is that there exists a body of literature concerning it of interest to psychologists. Thus, visual, auditory, and speech defects have been considered the proper material for this review, whereas mental deficiency and emotional disorders have not been included.

The aim for this review has been to select material which is basic to an understanding of the nature of the disability, and upon which the psychological literature rests. It is hoped that enough has been included to make intelligible the psychological problems involved.

BLINDNESS AND VISUAL DEFECTS

Those who work with the visually handicapped have felt the need for the evaluation of past efforts, the description of current research and its practical applications, the identification of areas in which research is needed and lacking, and the stimulation of interest among research workers. Two collections of papers, reviewing past accomplishments, present methods of dealing with the visually handicapped, and problems for future solution, have appeared during the past two years (1, 2). The first of these, edited by Paul A. Zahl (1), has the broader scope and deals with historical developments in many countries, with causation, with psychological appraisal, education, and vocational rehabilitation, and with aids, old and new, to communication and guidance. The other book's scope is adequately described by its title: *Psychological Diagnosis and Counseling of the Adult Blind* (2). The quality of presentation and the degree of penetration into important problems vary greatly from one contributor to another; those papers of particular value to psychologists will be referred to individually in appropriate sections of the following discussion.

¹ The period covered is from May 1, 1950, to May 1, 1952, approximately.

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Etiology.—Blindness has been variously defined and classified, and no single system of classification has been universally adopted; therefore, statistics on incidence are difficult to interpret (3). A general decrease in incidence in the United States, Canada, Great Britain, and Europe has resulted from the spread of preventive hygiene and the increasingly successful medical treatment of many of the injuries and diseases with which blindness is associated (4 to 8). In this country, regional differences are related to the age of particular populations, their racial composition, and the general health practices of areas compared (9).

During recent years there have been increases in the numbers of visually handicapped persons from several sources. In three groups, the increases are associated with higher survival rates: premature infants, diabetic adolescents, and the aged (4). In addition, two world wars have resulted in the blindness of servicemen from injuries and disease (3). Blindness caused by atomic bomb radiation has been reported for only a small number of cases up to the present time (3).

The increase in incidence of blindness among young children in the last decade has resulted largely from retrolental fibroplasia, a vascular disorder primarily affecting premature infants of low birth weights; hence, there is reflected in the literature a growing interest in the nature, possible prevention and cure of this disease. An excellent description of the course of the disease is given by Reese & Blodi (10). The acute phase of retrolental fibroplasia, during which there are some spontaneous recoveries, subsides by the end of three months after birth. The cicatricial phase which follows is not reversible and frequently results in partial or total blindness in both eyes. Relief through surgery in the latter phase has not been successful; therefore hopes for prevention and cure are concentrated upon the acute phase. Variations in the care given premature infants are being systematically observed, particularly in the administration of vitamin preparations and iron, believed by some to increase the incidence of the disease in certain hospital populations (10 to 13). Adrenocorticotropin has been tried on the theory that the disease is essentially an excessive growth of embryonic blood vessels, a growth which is normally inhibited during the last weeks of pregnancy. Successful treatment has been reported for a few cases (10). A baffling feature of this disorder is its higher incidence in the United States than in other countries, if the statistics are reliable, and the wide variations in incidence in different regions of the United States (11, 14). These variations may be partly due to differences in diagnosis, since retrolental fibroplasia is easily confused with several other disorders (10). Another suggestion is that it may be epidemic in nature (15).

Results of ophthalmological examination of 500 newborn infants, consecutive births in the University of Arkansas Hospital, are reported by Cook & Glasscock (16)—a contribution to the normative data on the condition of the eyes at birth. Although 10 of these infants were premature there was no evidence of retrolental fibroplasia in any of them. About one-fourth were

myopic in varying degree, and three-fourths were hyperopic. Astigmatism was more commonly associated with the latter condition. Detailed description of rarer abnormalities of the eye are given.

Several papers on the growth of the eyeball have been published during this period (17, 18, 19). Of particular interest is a comparison of the rates of growth of three dimensions—vertical, transverse, and axial—as reported by various investigators (19). The data suggest that the axial diameters increase more slowly than the others during childhood, although the proportional growth of the three dimensions appears to be relatively constant during both prenatal and adult life. The authors believe that this may in part explain the greater incidence of myopia found in school children, and the greater incidence of hyperopia at earlier and later ages. It is also suggested that, in eyes in which growth is attained early, there is a tendency toward hyperopia, the reverse being the case for eyes which grow more slowly.

Determination of the relative roles of hereditary and environmental factors in myopia continues to engage the efforts of several investigators. The conditions under which transmission is dominant or recessive in hereditary myopia are discussed by Wold (20). The facts that twins have varying degrees of myopia and that varying degrees of myopia are found in the two eyes of the single individual are taken to be evidence of the influence of environmental factors by one author (21). In another study, myopia was found to be more common among persons in occupations of higher socioeconomic levels, in which more education and greater use of the eyes can be assumed; however, no claim is made for a causal relationship between occupation and myopia (22).

One recurrent view is that myopia is increased by use of the eyes during school years. Kephart (23) has reported a study of 574 children in grades 1 through 12, in which 15 per cent of the first graders and 45 per cent of the twelfth graders were found to be myopic. The conclusion reached is that school work tends to encourage myopia. Eighty-four of the children were tested before and after the summer vacation; there was a reduction from 49 to 30 per cent. The author states that the tendency for recovery during the summer affected only the younger children—a conclusion not completely in accord with his data. Hirsch (24) tested 840 school children, 5 through 13 years of age, before and after the summer vacation. Hyperopia was more common among the younger children and decreased slightly during the summer months, but the rate of change was no greater than that for the rest of the year. Hence, Hirsch concludes that school work does not encourage myopia.

Cordes (25) has contributed an excellent nontechnical discussion of the visually handicapped school child, and the role of the ophthalmologist in cooperation with parents and teachers in his management. It is his belief that those cases of myopia in which there should be restrictions placed upon the use of the eyes can be identified, and the children's school life planned

accordingly, with a minimum of hampering psychological effects. He includes a brief but reasonably complete account of the causative factors of defective vision.

Recent claims for improved vision as a result of increased intake of vitamin A and calcium are not supported by a study of 72 cases of myopia and hyperopia given this treatment and followed for two years by Feldman (26).

A review and discussion of the literature on the causes and treatment of strabismus, published in 1950, has been presented by Burian (27). The discussion of causation deals largely with the pro and con of central nervous system versus retinal origin of strabismus. Changed handedness and emotional causes are mentioned briefly. Burian reserves judgment on the possible significance of abnormal electroencephalograms of strabismic children. Most authors agree that treatment should begin early, both for increasing the chances of achieving binocular vision and for preventing unnecessary psychological handicaps. Correction of refractive errors with glasses, orthopedic procedures, and surgery are the principal types of treatment. While there are varying opinions concerning the best types of treatment, all recognize the importance of the psychological management of the patient during treatment.

Nystagmus in coal miners has been a subject for speculation in recent years. One theory of causation has been that it is a neurotic manifestation. This view receives no support from a careful comparison of coal miners with nystagmus and other miners from the same locality (28). Both groups were given physical examinations, and medical, occupational, and social history data was gathered. In addition, a social worker visited their homes and talked with their wives. The men suffering from nystagmus were judged to be more stable than the controls but somewhat less intelligent and ambitious. Anxiety and depression resulted directly from financial stress and was not considered excessive.

Characteristics of the visually defective.—Psychological study of infants and preschool children with visual handicaps seems sadly lacking, as Maxfield (29) has observed. Particularly needed is more understanding of the nature and interdependence of the sensory processes at these early stages of development, and further research on the development of language in the preschool blind child. Testing at these early ages has not proved very successful up to the present time. The Interim Hayes-Binet is used with blind children from age four, but Hayes does not consider it reliable at the early age levels. An adaptation of the Vineland Social Maturity Scale has been found useful and worthy of further study. Studies of play equipment which encourage the development of the young blind child are sorely needed, as well as studies of the specific types of behavior problems which occur frequently at certain stages in his development.

Intelligence testing of the blind school child and the adult has been summarized by Hayes in two articles (30, 31). Hayes presents data to show com-

parisons between the 1930 Hayes-Binet, based on the 1916 Stanford Binet, the Interim Hayes-Binet, based on the 1937 Revised Stanford Binet, and the Verbal Scale of the Wechsler-Bellevue, Form I. Correlation between the two Hayes scales is similar to that reported between 1916 and 1937 Stanford Binets. The Interim Hayes-Binet, like the 1937 Stanford Binet, correlates higher with the Wechsler Verbal Scale than with earlier revisions of the scale itself.

With the exception of spectacular rises in IQ between first and second testing for some children who came to the Perkins Institution from extremely negligent homes, results on longitudinal intelligence testing of blind children are remarkably consistent. The fanning out of curves for different ability groups is similar to that of curves for children of different intelligence levels who have no handicaps. The mean IQ of the blind, regardless of the test used, is slightly lower than that of normal subjects, with somewhat larger numbers falling in the dull and the superior classifications. Hayes has found the Wechsler Verbal Scale and the Vocabulary Test suitable for the blind with very few modifications. He cautions against attempting to apply Wechsler's diagnostic patterns to blind subjects because of differences of experience. Few of the patterns can be applied at all because they depend on tests from the Performance Scale, which cannot be used with the blind.

Item analyses reveal that the blind do relatively poorly on arithmetic items in intelligence tests, possibly as a result of limited experience with the underlying concepts. This finding deserves further investigation; the facts as they stand present a challenge to educators, and further study might throw light upon concept formation in general. The blind perform poorly on the Digit Span Test during early years but excel in comparison with normal subjects at later ages; again, this is probably due to differences in educational emphasis for the blind.

Not enough data have accumulated yet on the Wechsler Verbal Scale, Form II, or the Wechsler Children's Scale, to make statistical analysis rewarding, but Hayes is of the opinion that these two scales will prove valuable additions to existing instruments for measuring the intelligence of the blind. Group intelligence tests, achievement tests, and personality inventories are of limited value, according to Hayes, because of wide variations in skill among the visually handicapped in reading Braille, and because of the expense of the materials.

Bauman & Hayes (32) have prepared a manual for use by psychologists who test the blind. Adequate background in general psychometrics is assumed; concentration is upon suggestions for meeting the special problems involved in testing and interpreting the test results on this handicapped group.

Bauman (33) also has a chapter on vocational tests for the blind in *Psychological Diagnosis and Counseling of the Adult Blind*. Testing devices for vocational appraisal of the blind are admittedly meagre, and additional tests which have been adapted for use with the blind and for which there are prop-

er norms are always welcome to vocational rehabilitation personnel. Preliminary results for the Purdue Pegboard Test indicate that this may be useful when adequate norms have accumulated (34).

Levine & Blackburn (35) compared performance of newly blinded soldiers on the Wechsler Verbal Scale with scores earned on the Army General Classification Test at the time of induction. The correlation is similar to that found between two intelligence tests of the same general type, administered to the same normal subjects ($r = .83$). The blinded soldiers made their best scores on comprehension and reasoning tests, the very tests on which Hayes has found that blind children become increasingly inferior with advancing age. The performance of the soldiers on the Digit Span Test was significantly lower than the mean of their weighted total scores. This finding is again in contrast to that usually found for blind adults handicapped since early childhood. It is suggested that the relatively poor showing of the soldiers on this test may reflect emotional disturbance interfering with concentration as a result of war experiences and the shock of sudden loss of sight. There was no evidence that loss of sight affects intelligence *per se* in adults. Those who had been blind for 34 months at the time of testing did not differ in performance from those blinded more recently. Such findings as these are in themselves interesting and useful, but their greater value is in encouraging further study of the mental test performance of contrasted groups, using more sharply drawn hypotheses concerning mental abilities and their organization.

The motor performance of visually handicapped school children was investigated by Buell (36) using the following tests: the 50-yard dash, the basketball throw for distance, the standing broad jump, and the Brace tests. The subjects were from 12 residential schools and 8 Braille classes in the regular schools. Factors considered were amount of vision, duration of handicap, amount of physical education, and parental attitudes. On the Brace tests, the visually handicapped children were below the norms on all items, and a third trial did not improve their scores; on most of the track tests, also, the handicapped made poorer scores than those made by normal children of like age and sex. An exception was the standing broad jump, on which visually handicapped children have more practice than is given to sighted children. Older children among the handicapped came nearer the norms for their ages than younger children, boys making better scores than girls. These differences, also, were attributed to differential amounts of practice. Those handicapped after six years of age made better scores than those handicapped earlier. Children from overprotective homes were less skillful than neglected children. The results of this study are in line with what is known concerning the effects of differential practice on motor skills. Its educational implications are obvious: more physical education for visually handicapped children, especially at early ages.

Brieland (37) investigated the speech of blind children, 12 to 18 years of age, using a control group of sighted children of the same sex, age, socioeconomic status, and rural or urban residence. The subjects were given a

story to read and study. Ten days later each was asked to retell the story. The retelling of the story was recorded, and movies were taken of part of it. Judges rated general effectiveness, vocal variety, pitch modulation, use of loudness, and memory on a five-point scale. The presence of defective sounds was noted. From the movie samples, general bodily action and lip movements were rated. To prevent the judges from knowing which subjects were blind, all of them wore dark glasses. The judges could not distinguish between individual blind and sighted subjects on the basis of their speech. The sighted subjects used more general bodily movement and lip movements; the blind were superior in pitch modulation and spoke more slowly.

Scientific studies of the personality adjustment of the blind are as rare as untested opinions are numerous (38). Jervis & Haslerud (39) studied frustration in three groups of adolescents: blind, sighted who were blindfolded, and sighted who were permitted to use their sight during the experiments. Six manual puzzles of increasing difficulty were the frustrating stimuli. Only half of the 10 physiological signs differentiated between the blind and the sighted subjects; differences between the blindfolded and other sighted subjects were negligible. The sighted subjects did more flushing, and more biting of lips and tongue; the blind did more sighing, and more rapid and uneven breathing. Verbalizations were classified by Rosenzweig's system. The greater intropunitiveness of the blind suggested that they lacked experience in expressing their emotions externally, and as a result were immature.

Bauman (40), using an inventory, studied the adjustment of three groups: blind, sighted, and persons with handicaps other than blindness. She concludes that the problems of the blind result from economic and social consequences of blindness rather than from physical aspects. Preliminary results of a study of visually handicapped school children, tested with an open-ended adjustment inventory printed in large print or Braille, suggest greater emotionality, anxiety concerning home relationships, and dissatisfaction with school, among the handicapped (41). Greater rigidity among blind children is indicated by a preliminary report of a study using an extensive battery of tests including projective techniques (42). The adequacy of the measuring instruments and testing procedures cannot be ascertained from the last two reports.

The preceding pioneering efforts suggest some of the factors which must be controlled before the effects of blindness upon personality development can be determined. By the use of blindfolded controls, Jervis & Haslerud (39) distinguished between the temporary frustration which could be produced in anyone confronted with a new situation without sight, and the cumulative effects of frustrations over the years which are experienced by the blind. Another factor that should be controlled is institutionalization, which may affect personality development. Whether or not the adjustment problems of the blind are different from those of other handicapped persons, a question touched upon by Bauman, deserves further study. It has been mentioned previously in this review that the results of personality in-

ventories printed in Braille must be interpreted with caution because of the relatively greater difficulty in reading Braille at best, and because of the wide individual differences in reading ability among the blind. Studies, such as that of Jervis & Haslerud, have the advantage of using materials which can be presented to all the subjects in exactly the same manner; results from such studies carry greater conviction than those in which presentation of materials varies from one group to another.

Since visual imagery may be retained in some degree by those accidentally blinded, while congenitally blinded have no opportunity to acquire it, a comparison of these two groups and of sighted individuals makes it possible to learn something about the role played by visual imagery in judging form and space relations. Worchel (43) reports the results of three experiments which deal with the relation of visual imagery to tactual form perception, manipulation of space relations, and space orientation. Thirty-three blind individuals, some congenitally blinded, and some blinded at later ages, were matched for sex and age with an equal number of sighted individuals. All subjects were blind-folded during the experiments.

The tests of tactual form perception required the subjects to reproduce by drawing, to describe verbally, and to recognize from among four similar but larger forms, a form which they had previously handled. The sighted individuals were superior to the blind in reproduction and description, and the accidentally blinded also were superior to the congenitally blinded on these two tests. None of the subjects made more than a few errors on the recognition test. Worchel concludes that the use of visual imagery accounts for the superiority of the sighted and the accidentally blinded on the reproduction and description of forms, and that the lack of discrimination between the groups on the recognition test indicates that visual imagery is not required for success on the latter test.

In the space relations tests, the forms of the Minnesota Paper Form Board Test were made into actual forms and parts of forms. Each subject was given two complementary parts of a form, one in each hand, and asked what form he thought would result if the two parts were placed side by side. Again the sighted made better scores than the blind, and the accidentally blinded better than the congenitally blinded.

The space orientation tests required the subjects to return to a starting point by a different route than that by which they had been led away. In the first part of this test, they were led along the two sides of an isosceles triangle and required to return along the hypotenuse; the procedure was reversed in the second part. The triangles were of different sizes and therefore the distances to be covered varied from trial to trial. The sighted were again superior, but the accidentally blinded made scores similar to those of the congenitally blinded. Both the sighted and the blind estimated the distance to be covered in walking time; the errors of the blind were errors of direction rather than of distance. Worchel concludes that the blind depend on cues other than those of visual imagery for space orientation—probably auditory cues which were excluded from these experiments. As the author

points out, this hypothesis can be tested by repeating the space orientation experiments with deaf-blind subjects.

From earliest times there has been a popular belief in an obstacle sense possessed by some blind persons. Three explanatory theories have recurred through the years: that the blind possess an occult or sixth sense not possessed by sighted individuals; that loss of sight is accompanied by sensitization of the others senses; and that the blind sometimes learn to respond to sensory stimuli commonly ignored by the sighted. Scientific study leads to acceptance of the last theory. Jerome & Proshansky (44) review the history of anecdotal and early scientific studies of obstacle perception in the blind. More recent studies have given information concerning the conditions under which obstacle perception is reported by the blind (45, 46). Worchel & Dallenbach (47), using deaf-blind subjects, demonstrated that the deaf-blind do not possess an obstacle sense.

Dallenbach and his associates believe that they demonstrated also the existence of an obstacle sense in some blind persons, dependent upon audition; recently Jerome & Proshansky (44) have corroborated their results. They gave four totally blind individuals the task of discriminating between situations in which obstacles were sometimes present and sometimes not; for the obstacle present series, they varied the distance of the obstacles from three to nine feet. Adjusted detection scores (successes minus failures) showed all four subjects possessed obstacle perception to a marked degree. Three showed a marked loss in accuracy with increased distance; one showed little loss of accuracy with increased distance up to nine feet. A second experiment required the subjects to avoid systematically varied real and "symbolic" obstacles (marked spaces the shape and size of the real obstacles) under two conditions: without sensory obstruction and with aural obstruction. Reliable differences were found between the ability to avoid real and symbolic obstacles, and between the avoidance scores under the two sensory conditions; moreover, there was no difference in avoidance of symbolic obstacles under the two sensory conditions. Obstacle perception reduced the errors (bumping into obstacles) by about 75 per cent. Jerome & Proshansky caution against assuming from this experiment that no other than aural stimuli (e.g., air currents and temperature changes) play a part in obstacle perception under nonlaboratory conditions.

Two more experiments give additional information concerning auditory factors in obstacle perception. Cotzin & Dallenbach (48) studied the relative importance of loudness and pitch. The sounds were reflection of footsteps and both thermal noise and pure tones picked up by moving microphones. Obstacle perception resulted from changes in pitch, not loudness, and continuous sounds gave better results than intermittent sounds. At normal walking speeds, a single pure tone of 10,000 or above (within the audible range) was sufficient for perception, although complex sounds were better. The authors conclude that the pitch changes perceived are due to the Doppler effect. A second experiment, by Worchel and associates (49) tested the distribution of obstacle perception in 34 of the 36 totally blind individ-

uals known to live in Texas. These subjects were given a series of tests out of doors similar to those carried on in the laboratory earlier. Those possessing obstacle perception were able to stop short of touching obstacles at remarkably small distances from them. The few errors they made were attributed to distractions from traffic, airplanes, and other city noises. Seven of the subjects who made similar scores on regular and catch tests were considered lacking in obstacle perception.

A detailed account of the work of the Committee on Sensory Devices, from 1944 to 1948, is given in the volume edited by Zahl (1). It is now apparent to all who collaborated on this project that physiological and psychological limitations of blind individuals render valueless many of the devices technology can produce. At best a technological device provides information of a very limited sort compared to that available to the sighted from visual perception. Among the devices developed by the Committee, two appeared worthy of further testing. Both of these were beam-probe devices, one using a supersonic beam and the other a beam of light. Each has advantages and disadvantages in practical situations. The final test of the usefulness of any mechanical device is that the blind individual using it can perform better with it than by depending upon his own trained obstacle sense. All the senses employed in mechanical devices require training. An extremely important research problem is to ascertain whether and to what degree the required perceptions can be acquired by different individuals.

The paper by Lashley (50) is particularly helpful concerning the understanding of the problems involved in making devices useful to the blind. He describes the slow development of perceptual abilities in general, even under conditions ideally motivated for learning, the limitations of the discriminatory powers of the auditory and tactile senses upon which the blind must depend, the psychological needs which require instruments of different types, and the necessary social adjustments to the use of instrumental aids. He makes a plea for enriching the environment of blind children by providing them with toys which encourage the development of useful sensory perceptions.

In 1950 there appeared an account of the reactions of a 40-year-old man who regained his sight after only eight months of blindness (51). The various stages of his comparatively rapid readjustment to the sighted world are of interest to those who would better understand the nature of the development of visual perception.

An exhaustive description of communication and guidance aids for the blind is given in Zahl's volume (1) and in a few other publications (52, 53). These include cane, guide dog, Braille, talking book, recording devices, and various methods of magnification of print for the partially blind. The comparatively rare instances where these devices have been subjected to controlled tests have been reported. Several papers in the Zahl volume (1) deal with the impressions of blind individuals who use various devices.

Treatment of the visually handicapped.—Because of the increasing numbers of visually defective young children, a recent description of the four

residential centers for infants and preschool children with defective vision and of outpatient clinical facilities is of special interest (29).

On the basis of extensive experience in this field, Lowenfeld (54, 55) has contributed several articles on the particular problems of the young blind child and present methods of dealing with these children at home and at school. The Forty-Ninth Yearbook of the National Society for the Study of Education, entitled *The Education of Exceptional Children* (56), contains descriptive accounts of educational provisions for children with defective vision and current philosophies concerning their education and training.

A popularly written history of the education of the blind, with special attention to the deaf-blind, and with an introduction by Helen Keller, appeared in 1950 (57). Two other nontechnical books published recently are concerned with the adjustment of the blind (58, 59).

The work of the rehabilitation centers for blinded servicemen in this country and in England, suggests unsolved problems which might well engage the attention of research workers (1). There is a heartening combination of optimism and realism in rehabilitation as it exists today (60). An especially urgent need is for a greater variety of occupational opportunities for the blind, and a change in employer attitudes. The research worker's contribution to such changes of attitude has not been fully realized. Attitude changes depend upon educational programs; the new information gained from research is vital to such programs. A review of current psychological theory, research methodology, and possible applications to work with the visually handicapped has been presented by Donahue (61).

DEFECTIVE HEARING

Although research on the deaf and hard of hearing is an extremely active field, there has been no systematic presentation of the various aspects of deafness which compares with those published during this period on the visually handicapped. As far as possible, the research which has appeared during the last two years will be related to the broader problems and trends of the last decade.

Etiology.—*The Forty-Ninth Yearbook of the National Society for the Study of Education* (56) includes a brief account of the causes of deafness. An hereditary cause which is receiving increasing attention is Rh incompatibility between mother and child. Authors vary widely in the frequency with which they report the Rh factor as a cause of deafness; however, all agree that it contributes substantially to the number of children with hearing losses (62, 63). Adults who have hereditary types of deafness, it is urged, should be educated concerning the probabilities of transmission to their children (64).

Among adventitious causes of deafness, the infectious diseases continue to be of major importance. Rubella (German measles), in itself a mild disease, is now known to result in deafness for some children whose mothers contract the disease during the first three months of pregnancy (65). Deafness is also a frequent sequel to the infections contracted by the child during

early years. The duration and severity of the infectious diseases of infancy and early childhood, as well as their mortality rates, have been sharply reduced by the use of the new antibiotic drugs (66). It remains to be demonstrated, however, whether there has been a reduction also in such sequelae as deafness (56).

Deafness has been reported by several investigators after treatment with streptomycin. Glorig (67) has made a careful study of the effects of streptomycin and dihydrostreptomycin upon the vestibular and cochlear divisions of the eighth nerve. Systematically varied dosages were given for a period of four months, and testing for damage was continued for a year. When 2 gm. of streptomycin was given daily for 30 days, damage occurred in the vestibular division of the nerve, and there was one case of hearing loss. When dosage was increased to 3 gm. daily, there was a sharp increase in hearing losses. Dihydrostreptomycin was less damaging to the vestibular apparatus but produced greater hearing loss. Damage did not always appear immediately—in some cases, not until several months after treatment had been discontinued. When it did occur, however, it was irreversible. Glorig concludes that these drugs should be used with extreme caution in the treatment of infectious diseases, and recommends that the dosage be restricted to 1 gm. daily for a period of two weeks. Carefully controlled testing of the effects of other antibiotics upon the sensory apparatus and upon other functions is needed.

A study by Fiedler (68) is concerned with the problem of preventive hygiene. She compared the families of children with hearing losses or diseases of the ear requiring treatment, and those of a control group who were matched for age, sex, and father's occupation. Both groups were below average in socioeconomic status and had similar numbers of contacts with social agencies. The families of the children with ear problems, however, had more frequently sought financial aid and health services. As a further check, the families of the latter group were compared with those of a matched group of children with dental caries. These two groups were similar both in numbers and in types of social agency contacts. Fiedler concludes that the neglect of the children with hearing losses and diseases of the ear is but one indication of the general underprivilege of their families, which constitutes a general community problem in health education.

An experimental contribution by Worchel & Dallenbach (69) deals with the vestibular sensitivity of the deaf. Two tests were given to 59 individuals with hearing losses of 32 to 100 per cent, 25 of whom were congenitally deaf. The first test consisted of standing on one foot for 25 sec., with three trials on each foot. Next the subjects were rotated while they sat in a chair; this was repeated with the subjects in three different positions. The criteria for determining whether or not each subject was affected by rotation were dizziness reported by the subject, nystagmus, and compensatory movements. Results on the two tests were then compared. All subjects immediately successful in standing on one foot were also affected by rotation, and those completely unsuccessful were not affected by rotation. Other subjects

presented a mixed picture. Degree of deafness was related to disability on both tests, and there were characteristic differences between the congenitally deaf and those handicapped at later ages. These authors conclude that the organs of the maculae and the semicircular canals subserve different functions; loss of one set of functions can occur without loss of the other; and loss of either may be partial or total. Further investigation along these lines may provide excellent diagnostic tests of area and extent of damage to the eighth nerve.

Results of audiometer and articulation tests, and the relationship of bone and air conduction, were found to be consistent with medical diagnosis of deafness by Newly & Molyneaux (70). Cases in the major categories of clinical diagnosis had typical patterns; the tests gave more detailed information.

Characteristics of the deaf and hard of hearing.—Berlinsky (71) has reviewed the literature on the measurement of intelligence, achievement, and personality of the deaf. Early studies of the intelligence test performance of the deaf found them markedly inferior when compared with normal individuals. More recent studies, using better methodology and better tests, find them only slightly inferior in mean test performance, with larger numbers of cases concentrated at the lower levels of intelligence. The results of comparisons of children congenitally deaf and those deafened at some later time usually find the former group to be inferior; however, in some studies no difference has been found. Oleron (72), reporting results for deaf children tested with the Raven Mental Matrices, found that they were inferior to normal children on this test of the ability to deal with abstractions, and that the congenitally deaf showed the greater inferiority.

Two studies have analyzed the data on deaf children retested several times on the same nonverbal scales (73, 74). Both investigators found that large increments resulting from practice effect made retests invalid, and they recommend the use of different scales for retesting deaf children. Similar results have been reported for normal children retested on nonverbal scales (75). While practice effect is also a factor to be considered in interpreting the results of retests with verbal scales, particularly when superior children are subjects, nonverbal retests are distorted to a greater extent.

The interpretation of test results of deaf children is difficult because the handicap in verbal facility may also influence nonverbal test performance. As Berlinsky (71) has pointed out, normal children may use subvocal speech in solving nonverbal tests, and there are few nonverbal tests from which can be eliminated the advantage of the normal child in gaining information from verbal sources.

Nonverbal tests have an even more serious limitation. Testing is usually undertaken for the purpose of classifying individuals and predicting academic success. A study using a number of different nonverbal tests illustrates this limitation (76). The subjects were a group of deaf children who were having difficulty with academic work. Results of their performance on the Leiter International Performance Scale were compared with results on the

Arthur Performance Scale, Form I, the Hiskey Nebraska Tests of Learning Aptitude for Young Deaf Children, the Performance Scale of the Wechsler Scale, Form I, and the Goodenough Drawing Test. The results on the Leiter and the Goodenough tests were similar but lower than the results on the other performance scales. The authors point out that the Leiter and Goodenough tests also compare more closely with the Stanford Binet, which is generally considered the most predictive of the verbal scales. It is suggested that further study be made of deaf children who show a serious discrepancy between performance on the Leiter or Goodenough tests and performance on the other scales. Such discrepancies may be predictive of lack of aptitude for academic work.

An excellent study of the reasoning of children with defective hearing has been made by Templin (77, 78). She compared carefully diagnosed groups of deaf, hard of hearing, and normal hearing children on reasoning tests adapted from the work of Piaget (79) and of Deutsche (80). The same proportion of children in all three groups came from residential and day schools. Residential schooling proved to have little effect upon the development of reasoning. In general, the deaf made the poorest scores, the normal children the best, with the intermediate position held by the hard of hearing. On a few tests the deaf excelled the hard of hearing because of differential training and experience. From this study, Templin concludes that defective hearing greatly retards the development of reasoning and that these handicapped children are markedly immature. Since the handicapped children made lower scores than those made by the youngest normal children in the study, the immaturity hypothesis should be checked by the testing of younger, normal children. However, the results of personality tests of deaf children indicate immaturity also (71).

The achievement of children with defective hearing falls below that of normal children at all ages, and increases with years of schooling (71). While children taught manually are usually inferior in achievement to those taught lipreading, often the manual method has been resorted to because of the severity of the handicap. No new studies of achievement or controlled experiments on new teaching techniques have appeared recently. In the face of the progressive retardation of this handicapped group, the importance of developing and testing new educational approaches can scarcely be overemphasized.

Most of the studies of personality find deaf children to be more neurotic, more often exhibiting problem behavior, and, as mentioned above, more immature than normal children of the same age and sex (71). A few have reported no difference between deaf and normal children. The areas in which the greatest amount of maladjustment is reported are those involving home relationships and social relationships in general. In one study, children from homes where there were other deaf persons were better adjusted than those who were the only deaf members of their families (81). Only one study to date has employed a personality measure originally constructed for use with the deaf (82). With the exception of a study using an adapted Rorschach

with deaf-mutes, which found them extremely low in productivity and in quality of response, there are no new studies of the personality of the deaf, although the subject is by no means exhausted (83). It seems doubtful that the Rorschach can be considered an appropriate test for subjects who lack speech.

Hearing tests.—Of special importance is the diagnosis of congenital deafness and of deafness acquired during the preschool years. By early diagnosis it is possible to give young children with defective hearing the special attention required to keep them mentally alert, and to provide them with the means of communication essential to normal personality development. Testing the hearing of children at these early ages is, however, extremely difficult.

Several authors have reviewed the techniques available for testing the hearing of infants and young children (84 to 87). Hardy & Bordley (84) discuss the advantages and disadvantages of the various techniques for testing them, including the psychogalvanometric skin response technique developed by Hardy and his associates. In another article, Hardy & Pauls (88) present the details of their procedure and of the psychological management of the children being tested.

The other tests include the observation of head turning and of palpebral reflexes, which were depended upon in earlier years, and those which require some form of motivated learning such as the "peep-show" test, which have been developed more recently (89). All these tests require the active participation and co-operation of the child, and are subject to variations in his interest, attention span, and reaction time. None of them is suitable for use with children under three years of age, and their results even at later ages are of doubtful reliability. Electroencephalography has been found useful in the detection of the absence or presence of hearing in psychogenically disturbed individuals, but it provides no information concerning partial hearing losses.

The technique developed by Hardy and his associates (84) involves the use of pure tone audiometry, mechanical recording of the lowered electrical resistance of the skin caused by the increased activity of the sweat glands after a mild faradic shock, and production of a modified form of Pavlovian conditioning with the shock as the conditioning stimulus. The reasonably comfortable child has a typical pattern of response which can be used as a base line, the disturbed child has no pattern at all, and the pattern of the cerebral palsied child presents special problems of interpretation because of the interferences of athetoid movements. The validity of the technique is dependent upon the normal functioning of the sympathetic nervous system. From 1 to 1½ hr. are spent with the child (88). In addition to obtaining medical and developmental data, the clinician may give the child developmental tests. Then the child is given free field testing of speech delivered over a loud speaker. After this preliminary information has been gathered, and rapport established, the psychogalvanometric technique is introduced. Infants are held on an adult's lap, older children are placed in a high chair or a small chair in front of a low table, while the electrodes are being fastened to feet

or legs. Children over 18 months of age are kept quiet and interested with small toys. No restraints are used, and no appropriate sedation has yet been discovered. Children of four or five are the most difficult to manage, and are occasionally resistive. The youngest child successfully tested to date was four months old.

The Hardy method requires a team of two persons, experienced in dealing with young children as well as the technicalities of conditioning. Since the equipment is expensive, the method is limited to use in medical centers. Although some of the details of conditioning are still under investigation, 650 infants and young children have been tested, and the results of retests and comparisons with other techniques are encouraging.

For a number of reasons, a young child may hear a sound but fail to respond to it. One reason may be that the sound has no meaning for him. Myklebust (85), in his discussion of the problems of testing young children, urges research upon the normal development of auditory perception during early years, and suggests that the psychogalvanometric skin technique may be the most suitable for this purpose. For practical purposes he favors the use of free field testing with speech, in the belief that pure tone audiometry gives less information that is useful in the planning of treatment.

There have been several reports of variations in the techniques of testing school children with both individual and group tests. A study of Danish children, 6 through 13 years of age, is described by Nielsen (90). The children responded to groups of four pure tones, some short and some long, by recording dots and dashes. Nielsen believes that this method of recording eliminates guessing and discourages day dreaming. The results of a testing program in which this technique was used to detect deafness, in children referred to hearing specialists for treatment, revealed many more defects among the younger children and that their defects were usually mild. The program's effectiveness in preventing serious defects in older children was tested by comparing the 11-year-olds of this school system with children of the same age from schools which did not have such a program. The older children of the latter schools were found to have five to seven times more hearing defects.

Another group test is presented by Bennett (91) in a study of 500 six-year-olds. The test consists of rows of pictures of objects, the names of those in a row having similar phonetic sounds. The children were instructed by phonograph to mark one of the pictures in each row. They were subsequently tested on the pure tone audiometer, and the results of the two tests compared for each child. Of 56 children found to have hearing losses of 15 db. or more on the audiometer, 54 also failed the speech test. However, 25 per cent of the children who failed the speech test had normal hearing according to the audiometric results.

Children of six years are extremely suggestible, have not yet learned to work independently in groups, and may not be able to make the discriminations required by the Bennett test, even though they hear the instructions. It seems likely that group tests applied to children of this age will continue

to select many children with normal hearing as well as to miss an occasional child with defective hearing who manages to copy some other child's responses. Considering the importance of early detection and treatment of defective hearing, to miss even one child is a serious matter. If the alternative is no testing program at all, group tests such as those described by Bennett and by Nielsen are well worth giving. It is to be hoped, however, that eventually an individual diagnosis of sensory functioning will be a routine procedure at school entrance.

Curry (92) demonstrated the superiority of audiometric testing over teachers' ratings of defective hearing. He found that teachers miss a large proportion of the children with defective hearing according to audiometric tests, and that they also suspect deafness in many children who have normal hearing. Such discrepancies are to be expected, since teachers have an opportunity to observe only the child's responses to a limited range of speech frequencies, and because inattention in the classroom has many causes besides defective hearing.

One other testing device is worthy of mention. This is an "ear-choice" technique, in which the subject responds to pure tones by first indicating whether he hears it, and then in which ear he hears it (93). The clinician varies the tones and the ear into which the sound is sent. The second response is a good check on the reliability of the subject's report. The technique compares favorably with others on adults. It has not yet been tested experimentally on children, but its simplicity suggests that it may be useful with them.

Treatment.—Several authors plead for closer co-operation among the different specialists who work for the rehabilitation of the deaf and hard of hearing. Lesser (94) points out the futility of audiometric testing unless it is followed by remedial work with those found to have defective hearing. Reiter (95) warns of the danger of overemphasizing the value of hearing aids. Other forms of therapy which have the purpose of teaching the deafened to use their residual hearing, and to sharpen their use of other sensory cues, should not be neglected. Also, hearing aids cannot benefit all persons who have defective hearing.

A study by Weaver (96) shows that many children who have been fitted with hearing aids fail to use them. He found no relationship between the extent to which the children used their aids and the severity of their hearing losses, the amount of hearing gained by the use of the aid, or the length of time they had had their aids. This suggests that there is still much to be learned concerning ways of motivating children to get the most out of their hearing aids. The preceptions which must be acquired for effective use of an appropriate aid are developed slowly and only with the encouragement of a program of education (97).

Hirsh (98), reporting results of earlier experiments on the relative efficiency of binaural and monaural hearing aids, concludes that differences between monaural and the usual binaural hearing aids, which merely send sound from the same source to both ears, are too slight to be of importance to

the wearer. In recent experiments Hirsh has found that for intelligibility of speech and accurate location of sound the best results are achieved by a separation of the sources of sound—i.e., a binaural system is superior only if the individual is supplied with two aids so that he can hear sounds from two sources. The new type of aid is called a stereophonic aid.

Carhart (99) discusses the role of the audiologist in university clinic with respect to the fitting of hearing aids. About 80 per cent of the people who can be benefited by aids can use any of the good ones now available. The other 20 per cent have individual requirements which make certain types of aids more beneficial. By individual diagnosis failures are avoided and time is saved for all concerned. The clinic has the responsibility of discouraging efforts to use aids by those who cannot benefit from them.

One study has attempted to discover the attitudes of employers toward hearing aid wearers (100). Though the study lacks precision, the following tentative conclusions are of interest: employers are more willing to accept the deaf for employment than other handicapped groups provided they wear hearing aids, but are unsympathetic toward those who need aids but will not wear them; there is some selection of the type of employment offered the hearing aid wearer, based on the individual employer's preconceptions as to suitable employment; employment is largely based on ability and personality without regard for the handicap of the hearing aid wearer; and employers are, for the most part, unrealistic about the limitations of the deafened even after they have been fitted with and adjusted to hearing aids.

SPEECH DEFECTS

Dysarthria.—In a discussion of the etiology and differential diagnosis of various forms of dysarthria, Peacher (101) includes disorders of articulation, phonation, resonance, and respiration. Incidence is estimated at 2 to 3 per cent of the population. If cerebral palsied persons with speech defects and the many neurological cases not usually referred to speech specialists are also included, incidence is probably higher. After describing recent studies of speech disorders correlated with the removal of brain tissue, he quotes Hughling Jackson's famous dictum, "To locate the damage which destroys speech and to locate speech are two different things."

Renewed interest in the location of speech centers is reflected by the translation of the original article on the subject by Broca, in which he reviewed the opinions held at that time, and illustrated his own with a case study of an aphasic (102). Brown & Schuell (103) have developed a test for aphasics with which they hope to accumulate psychological data which can be related to neurological findings. Preliminary results from this test lead them to believe that all parts of speech are affected in aphasia, contrary to the views of those who describe a special form of aphasia for nouns only. Although the impairment of one area was greater than that of another in individual cases, some impairment of both sensory and motor elements was present in all cases. Those who had difficulty with auditory comprehension benefited least from therapy.

Schuell (104) has interested himself, also, in the nature of paraphasia and paralexia. After presenting a comprehensive review of the opinions held by various authorities, he reports the results of an analysis of the speech of 15 paraphasic patients. Samples of their speech were classified according to the system developed by Jung & Riklin (105) for the study of free association. The subjects of Jung & Riklin were instructed to say the first word they thought of; Schuell's patients were attempting to say the "right" word. In spite of this difference in purpose, there were marked similarities between the two sets of responses. Schuell concludes that the major difficulty of the paraphasics was in making an appropriate selection from among the many responses available to them. Impairment appeared to have occurred at different stages of the thought process in different patients. As they improved under therapy, they were able to inhibit inappropriate responses and to express appropriate ones. Although this study was not a controlled experiment, the results provided a synthesis of the views of many previous observers, and suggest an extremely interesting approach for the further investigation of paraphasia.

Many possible causes of delayed speech or congenital lack of speech are mentioned in the literature. They include feeble-mindedness, deafness, structural anomalies of the organs of articulation, paralysis of the speech organs, failure of development or injury of the speech areas of the brain, rachitis and malnutrition, and emotional problems in the home (106). Determining the cause of language retardation in a specific case is sometimes difficult. Goldenberg (107) reviews the opinions, largely untested, of many who have written on the subject, and then presents the results of a study of 14 cases to whom he administered a battery of psychological tests. Four cases were diagnosed as feeble-minded, two as deaf, and two as extensively brain-injured rather than aphasic. The etiology of the remaining six could not be determined. Many of the tests used by Goldenberg are of unknown validity. Because of the numerous possible causes of language retardation, it appears that the combined efforts of a number of different specialists may be required to determine the factors operative in some cases.

Cases of poor articulation for whom no major organic cause has been found are usually designated as "functional." Fairbanks and his associates (108) have pointed out that the distinction between organic and functional cases is not always clear. They have undertaken the measurement of the organs of articulation of subjects with contrasted ratings on articulation tests, none of whom had gross organic defects or other speech defects. To date reports have been made on the rate of movement of the oral structures, the dimensions and relationships of the lips, the tongue, the teeth, and the hard palate (108 to 111). The only significant difference found between those with good and poor articulation was the greater number of dental deviations of marked degree among the poor speakers, of which the most frequently observed condition was openbite (111). The fifth article of the series, not yet published, is to report measurements on the hearing of the good and poor speakers, and will include a general discussion of all the findings.

Various methods of repair of cleft palate by surgery, and use of dental prosthesis in some cases, have been described (112 to 114). Most authors urge early repair, in order to avoid poor speech habits and to improve the child's appearance as soon as possible. However, Graber (112) warns against surgery before four years of age and favors the growing tendency toward the use of prosthesis. McDonald & Baker (115) made a careful study of the speech problems of the individual with a repaired cleft palate, drawing from both the clinical and experimental literature. As a result of their analysis, they conclude that there should be certain changes in therapy.

There is certainly a more hopeful outlook for aphasics now that greater emphasis is given to associated auditory defects and to both sensory and motor elements. Case histories published recently demonstrate marked improvement in several cases of long standing, and the amazing recovery of a student who returned to his studies to achieve honors at his university (116 to 118). Wepman's *Recovery from Aphasia* (119), although somewhat sketchy with respect to etiology and differential diagnosis, is encouraging in its description of current therapeutic procedures and the resulting improvements and cures. A bibliography of the English literature on aphasia, covering the period from 1864 to 1951, includes 388 published titles and 21 dissertations (120).

Stuttering.—The etiology of stuttering remains controversial, although many accept the theory held by Johnson (121) and others, that it is the result of anxiety engendered in early childhood by setting too high standards of speech. In reviewing the literature on laterality, Hildreth (122, 123) presents two contrasting views concerning causality: (a) that stuttering may be part of a general motor clumsiness about which the child becomes emotional because of the reactions of others; and (b) that stuttering is a result of emotional disturbances and is, therefore, a form of neurosis from the beginning. She finds little evidence that stuttering is produced by changed handedness, and believing that righthandedness is a great advantage outlines procedures by which it can be taught to young children without arousing their resistance.

In search of a possible organic basis of stuttering, Boland (124) interviewed the mothers of stutterers about the conditions attending their deliveries. Twenty-two per cent reported instrumental delivery, a proportion somewhat greater than that usually reported for the general population. Assuming that faulty memory reduced the number of instrumental deliveries reported, by mothers of stutterers, Boland corrected his data by one-third in the direction of his thesis, and thereby produced a "statistically significant" but psychologically questionable difference!

A developmental relationship between cluttering speech and stuttering has been suggested by several authors (125 to 127). Cluttering, an exaggeration of the usual errors of speech, is frequently associated with general motor clumsiness or delayed motor development. It is presumed to have an organic basis. The clutterer differs from the stutterer in several important ways: he is easy-going, careless, and capable of improving his speech by conscious effort; while the stutterer is tense, cautious, and frequently unable

to improve his speech even after extensive therapy. It is now believed by some that stuttering develops from cluttering if a child is under severe environmental pressure (125).

The existence of stuttering in both members of twins has been offered as evidence of an hereditary basis for the disorder. Recently, however, a distinction has been made between genuine and imitative stuttering that permits another interpretation (128). Imitative stuttering differs from genuine stuttering by omissions and reversals of the usual sequence of development. Brodnitz (129) now suggests that the stuttering of twins may be explained by the imitation of one twin's genuine stuttering by the other member of the pair, and presents a case study which illustrates this possibility. Further study of the exact nature of stuttering in twins is in order.

Increased attention is being given to the social climate in the home as a precursor to the development of disorders of all kinds. Ross (130) reports a study of the improvement of speech in stutterers, some of whose mothers were interviewed by social workers and some of whom declined this form of therapy. The children whose mothers went untreated improved the most. The author believes that those children who improved had the advantage of better parent-child relationships.

In an effort to obtain extensive and specific information concerning the parental management of stuttering children, Moncur (131, 132) administered an interview of 330 items to the mothers of matched pairs of stutterers and nonstutterers. In general, the stutterers were subject to many more adverse factors in their homes. Since parental domination appeared frequently in interviews with mothers of stutterers, the author made a further analysis of the items concerned with four types of domination: disciplinary action; oversupervision and overprotection; holding the child to excessively high standards; and adverse criticism. On 47 per cent of the items there were significant differences in the direction of greater domination of the stutterers, and on most of the remaining items the trends were in the same direction. Moncur concludes that no one specific factor in the management of young children predisposes them toward stuttering, but that stuttering more frequently develops in an atmosphere of excessive parental domination.

Bloodstein (133, 134) interviewed adult stutterers concerning conditions under which their stuttering is reduced or absent. A great diversity of situations were described, and no one situation was mentioned by a majority of the subjects. Nevertheless, it was possible to group the situations mentioned into six categories by making inferences from the interviews. These conditions were: changes in speech patterns, associated activity, intense or unusual stimulation, reduction of responsibility for communication, reduction of the need to make a favorable impression, and absence of unfavorable listener reactions. The first three were considered to be situations in which the stutterer was distracted from his own speech, and the last three situations in which he felt free of anxiety. Carrying generalization one step further, the author interprets his data as supporting the theory that stuttering arises from the individual's efforts to avoid nonfluency, for which he has been

punished. Since anxiety is learned in situations which differ from child to child, it follows that the conditions under which stuttering is reduced or absent will be individual also. The results are plausible; they furnish hypotheses to be tested by more precise methods.

Assuming that stuttering is learned behavior, and that an emotional component of stuttering is anxiety, Wischner (135) reviews some of the research that has been published from this point of view, and attempts a systematic theoretical formulation as a basis for his own work. There are many similarities between the progressive reduction of stuttering with each repetition of the same material and the usual reduction of errors in conditioning experiments. Stutterers can predict the words on which they will block and the severity of their blocks. The longer the interval between the presentation of a stimulus and the signal to respond, the greater the tendency to stutter. Hence, application of learning theory leads to the conclusion that stuttering fulfills an expectancy and reduces tension although it does so at the price of normal speech.

In terms of the concept of reinforcement, stuttering is perpetuated because it not only fulfills an expectancy but gets the individual out of a painful situation, that is, completes the word, and is therefore rewarded. An example of the research which grows out of Hullian learning theory is a study by Sheehan (136) which attempts to test the hypothesis that reinforcement can be used to reduce as well as to perpetuate stuttering. Twenty subjects, serving as their own controls, were asked to read passages under two conditions. One condition required the simple repetition of the reading of the passage; the other condition required the subject to continue to repeat each word upon which he stuttered until he had repeated it correctly. There was an interval of 30 min. between the sixth and seventh trials. Stuttering was reduced more sharply under the experimental condition, that is, by the repetition of each word until expressed correctly. The difference between conditions in the reduction of stuttering was still apparent on the seventh trial. Sheehan suggests that the child whose parents fail to reinforce the correctly expressed word, rather than the child whose parents attempt to correct his speech, may be the child who becomes an habitual stutterer. Further research of this kind promises other new therapeutic techniques as well as explanations for the success of some already in use.

Two studies which deal with the personal and social adjustment of stutterers make use of the *Q-sort* technique. Spriestersbach (137) reports preliminary results of a study of the evaluation of pictures using words considered to be measures of social adjustment. The test was given to 20 psychotics, 50 stutterers, and 183 normal university students. The stutterers and the students differed in the expected direction from the psychotics in their evaluative responses. A smaller difference, but one significant at the 5 per cent level of confidence, existed between the stutterers and the students. The author tentatively concludes that the stutterers show a relatively mild degree of social maladjustment. One of the uncontrolled factors, however, was the severity of stuttering. Fiedler & Wepman (138) compared a small

group of stutterers with an even smaller group of nonstutterers on a self-concept rating scale, based on Murray's 76 personality traits. The scale failed to differentiate between the groups. Composite scales of the typical ratings of stutterers, mental hygiene patients, and clinical psychologists, showed the stutterers to possess self-concepts similar to those of the clinical psychologists. The authors conclude that the data do not support the view that stuttering is a serious detriment to normal personality development. It is possible that the self-concepts test reflects social stereotypes rather than the personalities of the raters; another possible interpretation is that stutterers achieve normal personalities by the use of different mechanisms than non-stutterers—for example, by compensatory processes.

Therapy for stutterers continues to reflect the diverse theoretical views of clinicians. Specific procedures vary but all clinicians attempt improvement of the patient's mental hygiene. The next step forward will be trial in the clinical situation of techniques suggested by the learning theory experiments.

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EDUCATIONAL PSYCHOLOGY¹

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INTRODUCTION

Definition and scope.—Educational psychology is here defined as that branch of psychology which deals with school learning and its correlates. The elaboration of this definition is implied in the content of the review. We may emphasize, however, that it is limited to the scientific psychological studies, and therefore leaves out of consideration much that is published in other approaches to education.

Selective factors.—This review is limited, with very few exceptions, to publications appearing in the twelve months preceding May 1, 1952. Selection of material has inevitably been influenced by the judgment and special interests of the reviewer. Nearly all publications have been omitted when not supported by data carefully collected and suitably presented. Finally, space requirements have made it necessary to exclude many worthy contributions, and no doubt others have been inadvertently overlooked.

Views regarding content.—Implications as to content are revealed in recently published articles on the teaching of educational psychology. The idea that educational psychology should concern itself extensively with studies in child development has received marked emphasis in the writings of Baller (6), Rivlin (99), and Murphy (87). Both Stone (110), and Horrocks (64) emphasize such topics as learning, emotional and social development, interests, individual differences, abilities, measurement, and evaluation. These topics are given major consideration in this review. In general, textbooks, compilations, and reviews are excluded. The recently published text by Stephens (109), however, deserves mention. This excellent textbook presents a balanced view, based upon clear knowledge of developmental trends in educational psychology.

READINESS FOR LEARNING

The existence of developmental sequences in learning has been demonstrated in a number of studies: by Ilg & Ames (66) in arithmetic, by Ames & Ilg (3) in handwriting, and by Springer (107) in connection with concepts relating to the clock. These studies all reveal sequences of behavior useful in diagnosis and instruction.

Ilg & Ames (66) seem a bit too ready to accept the idea that the processes taught in arithmetic grade for grade are too difficult. The views of Brownell (19), that one may alter method instead of grade placement, and distinguish

¹ The following abbreviations are used in this paper: MMPI (Minnesota Multiphasic Personality Inventory); ACE (American Council on Education).

between introduction and mastery of a topic, should be given more emphasis.

In dealing with grade placement of pupils, Fifer (45) presents evidence that sex differences in maturity are important determiners of readiness for the school curriculum. In current practice, relatively rigid promotional policies take little or no account of these differences. Moreover, those same inflexible policies prevent the acceleration of bright pupils. Bossard (17) investigated 100 case records, showing that social visiting away from home tends to improve a child's readiness for school, through giving the child perspective, needed information, and increased confidence.

Burnett's article (22) on the teaching of general psychology indicates a felt need on the part of most college students for instruction in how to study. They have a general lack of readiness for any instruction that is advanced or technical. They have an emotional readiness, however, for more up-to-date and informal methods of instruction. Stanley's study (108) of college freshmen reveals that they have greater insight into their own aesthetic, economic, and religious values than into their social or political values. More exploration of this type would be welcome in educational guidance.

SCHOOL LEARNING

Acquisition of desirable attitudes.—The idea that attitudes are important determiners of achievement has won widespread acceptance. It is shown, for example, in Meder's discussion (84) of the science curriculum. Many reports reviewed therein show the recent trend toward concern with scientific attitudes, as well as with the facts of science. There is also emphasis upon achievement of understanding and promotion of critical thinking.

A publication by Krathwohl (72) appears to show that work habits of industriousness are significant for prediction of success in engineering. The article is one of a series in which an error in statistical technique is involved. The only thing demonstrated is a very commonplace correlation between two measures of achievement.

Two studies offer suggestions as to what to do in order to bring about desirable changes in attitudes. The first, by Centers (24), deals with promotion of insight into one's attitudes toward national and other groups through a classroom demonstration of stereotypes. In the other study, Taylor (113) applied the Ohio Social Acceptance Scale in two eighth-grade classes in conjunction with a Guess-Who test. After identifying children with problems, the teachers employed group guidance techniques for improving the social acceptance of pupils who were isolated or unpopular.

Carter's analysis of study techniques (25) shows that attitudes are as significant as skills in the prediction of scholarship. It demonstrates that good morale, feelings of self-confidence in the school situation, and acceptance of the values characteristic of scholars are major determiners of school achievement.

Learning as a group process.—The book by Cunningham and others (31)

gives a general exposition of group techniques for use by teachers and other leaders. Two articles by Jean Grambs (55, 56) demonstrate the use of group methods in high school classes. Some superiority of group techniques of instruction is shown in a study by Levine & Butler (74), who dealt with a situation in which supervisors needed to change faulty practices in rating factory workers. The lecture technique was ineffective, while the group decision method led the supervisors to make the needed changes.

Meaningful learning.—A widespread interest in meaningful learning may be found in curriculum studies like that of Meder (84), mentioned above, and in studies of readiness, already reviewed. Buswell's article (21) on teaching arithmetic for understanding indicates the historical trends which have led to the present emphasis. In teaching for understanding, the principles of foremost importance seem to be a primary emphasis upon concreteness, followed at the appropriate time by progress into the teaching of generalizations and theory. The significance of number theory is pointed out. The absence of the common oversimplification in terms of later grade placement of materials is refreshing. Luchins (77) and others have recently been interested in the use, rather than the avoidance, of abstract processes. This tendency seems to be a desirable extension of thought concerning meaningful learning.

Prediction of school achievement.—Thompson (114) developed tests for measuring spelling in each grade, using only words taught in the given grade and included in the most basic words as shown by the Horn list. Such procedure calls attention to factors underlying the prediction of achievement and may serve to remind us of the theory of mental organization implied in the activities of curriculum building.

Robert L. Thorndike (116) performed a statistical analysis of achievement test, intelligence test, and census data, investigating the relationships between community variables and test results. The measures correlating highest with intelligence of children were those indicating the educational level of the adult population of the communities. The correlations with community variables were lower for achievement tests than for intelligence tests. It is interesting to compare the whole study with reports like that of Eells and co-workers (40) which appear to show a cultural bias in intelligence tests.

Two studies present evidence of personality characteristics useful in predicting success in nursing schools. According to Healy & Borg (60) the successful nurse tends to be stable emotionally, cheerful and optimistic, even though introverted and socially submissive. While being agreeable, cooperative, and objective, she is also highly conscientious. These findings are in general agreement with those of Petrie & Powell (95), who found that ability to get along with people is one of the special and distinct requirements for success in nursing. In addition, Petrie & Powell (95) stressed the importance of an intellectual factor which included tests of knowledge and of accuracy in perception as measured by clerical tests.

Various studies of prediction of success in professional schools have been made, with results tending to follow the usual pattern. Pierson & Jex (96)

found that the best prediction of success in engineering was obtained using a combination of high school marks with measures of achievement in English and mathematics. Glaser (49) found intelligence tests and reading tests significant for predicting success in medical school. The MMPI¹ showed no relationship with achievement. Grades in physiology were more useful than grades in other areas in predicting achievement in medical school. Weiss (127) showed that moderately good prediction of grades in dental theory courses could be obtained using a combination of predental grades and some sections of a science test. In predicting success in dental technique courses, tests of spatial visualization and of manual dexterity were also useful.

These studies reveal the complexity of prediction problems and the need for taking account of the local setting. Emphasis is placed upon the functional significance of abilities tested, as contrasted with the apparent or superficial importance of particular variables.

SOCIOLOGICAL CORRELATES OF LEARNING

National cultural differences.—Throughout the past 40 years, much attention has been paid to cultural differences associated with national and racial origins as factors affecting school adjustment. Several contributions bearing upon this problem have appeared within the past year. Barschak's study (9) comparing girls in four societies indicated differences in attitudes toward education. English girls seemed to have more faith in the importance of subject matter, examinations, and discipline. German girls emphasized the effect of personalities of teachers. Swiss girls were more critical of teachers and of scholarly demands. American girls seemed more impressed by the importance of "democracy in education," and considered "other values in education" more important than the academic side of school life. The findings are tentative, and in need of support from additional investigations. Farber (42) studied certain differences between English and American people by comparison of persons in the same occupational groupings. The results suggest a greater American tendency to exploit the environment, and greater concern of the English with inner personal life. The results are strictly tentative because of the limited numbers of cases and lack of sufficient control of selective factors affecting the samplings.

Hsu's study (65) comparing Chinese students in America with a similar group in China suggests that self-report inventories, when scored for such traits as neuroticism, are not valid when used with persons of different cultural backgrounds. Smith & Vinacke (106) investigated sense of humor in different racial and national groups in Hawaii. Marked differences in reactions to jokes were seen in comparing younger and older generations and in comparing Caucasians, Japanese, and Chinese. This study, along with others cited, helps to indicate the pervasive character of cultural differences affecting education.

Attitudes toward social groups.—American children take pride in the fact that they are Americans; groups of different origin vary in status in our

schools. According to Zeligs (129), who studied the nationalities children would choose if they could not be Americans, the English are the most popular of the other national groups. The most interesting aspect of the study is the indication of complexity in the reasons for preference. The detailed facts provide suggestions for instruction in social studies classes, within the framework of democratic school policies.

Several studies may be listed as contributing evidence as to the nature and development of prejudiced attitudes. Chyatte *et al.* (26), used an interview technique in an investigation of prejudices expressed in verbal comments of children. The work of Johnson (69) and of Cooper (29) indicates that prejudice is complexly related to knowledge. According to Cooper, in ego-involved situations prejudice is accompanied by claims as to a high level of knowledge, while in non-ego-involved situations, prejudice is associated with reported lower levels of information. Johnson (69) investigated prejudiced attitudes as shown by a projective test, finding bias associated with superficial knowledge of a culture. A curvilinear relationship indicated that least bias occurs when one is either very ignorant or very well informed. A factorial study of cultural patterns in the United States completed by Hofstaetter (62) showed that urbanization, better education, and racial discrimination are largely independent. The various studies tend to agree in showing that economic factors are apparently not major determiners of prejudice. High frequency of members of minority groups in a given community tends to be associated with increase in racial or minority group prejudice. Studies like these, contributing to knowledge of the nature of prejudice, help to form a better foundation for school programs designed to bring about more appropriate relationships among groups.

Social class experience.—The book by Eells (40) and his collaborators presents evidence for the claim that modern group tests of intelligence have a bias favoring middle-class and upper-class groups. Data from large numbers of children aged 9 to 10 and 13 to 14 are analyzed. Results appear to show that the lower social classes are handicapped by the tests, especially when the tests emphasize verbal abilities. This reviewer feels that the data are not adequate to support all the claims made. When certain statistical errors in the report are allowed for, some of the results will require more moderate interpretations.

An article by Swanson & Jones (112) reports some facts about ownership of television sets. In a Minneapolis sample, owners of television sets attended fewer movies, spent less time listening to radio, read fewer books, and spent less time with newspapers than nonowners. Owners of television sets knew less about current governmental affairs. They did not differ significantly from nonowners in years of education, amount of social activity, or intelligence. The difference in income, although not statistically significant, seemed large enough in favor of the television owners to have practical significance. The study is limited somewhat by the fact that the area sampled was at the time not fully developed as an area for the reception of telecasts.

EMOTIONAL FACTORS IN LEARNING

Interests.—Following the modern viewpoint, that stable characteristics of the individual's personality have to be reckoned with in the learning situation, psychologists have been investigating the stability and permanence of such traits as interests and their interrelationships with abilities and achievements. With a group of college students, Reid (97) has shown that interests as measured by the Kuder inventory remain fairly stable over a fifteen-month interval. When further analyzed, these group results yielded evidence that some individuals' interests are more predictable than those of other persons. Gustad's study (59) using the Strong Interest Blank and the ACE¹ test showed no consistent pattern differences in the interests of those with primarily linguistic or primarily quantitative abilities. The hypothesis that vocational interests are determined by differential aptitudes was rejected. Brown (18) found that expressed interests are not in good agreement with inventoried interests, and concluded that preference tests must be used to supplement statements of interests in the counseling or selection situation.

Attitudes and personality.—The last year has revealed continued concern with the relationships between attitudes and learning, resulting in a variety of studies. Neidt & Merrill (88) found a correlation of .36 between grade point averages and a scale for measuring attitudes toward education. Lowell (76) used a projective test to measure need for achievement, which was then shown to be significantly related to learning in laboratory tasks. Kausler (71) showed that an ego-involved group performed better in a perceptual learning task than did a control group matched for ability. A study by Dailey (32) demonstrated that the formation of premature conclusions interferes with subsequent learning in acquiring understanding of people. These investigations and others like them have clearly demonstrated that attitudes are important determiners of achievement.

A more specific expression of effects of internal limiting conditions was investigated by Patten (93), who was concerned with lack of improvement in written expression among college students in writing classes. Data were obtained from four samples of writing and from administration of the MMPI. Paucity in written expression was associated with a depression pattern of MMPI scores, while prolixity was associated with manic tendencies.

A final report for this section is that of Walter & Marzolf (122), who concluded that boys have higher levels of aspiration than girls of the same ability level. The authors suggest that the school is more likely to reward the "feminine" virtues of docility and submissiveness; hence boys are likely to be more often frustrated. Grades 4 through 12 were investigated, using a skill test dealing with accuracy in hitting a ball. The study needs the support of similar investigations using other types of tasks.

Adjustment.—A recent trend has been toward lesser attention to intelligence as a determiner of learning and toward increased concern with the effects of various types of maladjustments upon behavior of the sort which the schools seek to promote. Fey's study (44) of psychotic and normal per-

sons showed that the maladjusted group were clearly inferior in a performance task, although the groups were matched in age, intelligence, sex, and educational level. The maladjusted group had more difficulty in solving a problem assumed to require effective utilization of generalizations and concepts.

Grace (52) studied types of hostile reactions, finding that the subjects, graduates in education, tended to react habitually, showing types of hostility more dependent upon individual traits than upon types of external situations. Hinkelman (61), who regarded intelligence as the cognitive aspect of capacity for adjustment, found that bright pupils get better adjustment scores on the California Test of Personality. Hinkelman's study, of course, needs to be repeated in other situations, with more subtle measures of adjustment.

The work of Luchins (77) using the Einstellung test of rigidity is especially interesting. Concreteness of thinking is thought of as a symptom and a determinant of rigidity. Rigidity is indicated by perseverative response in the form of repeated solution of problems by a given habitual method when a simpler and generally more flexible method could be used. While it seems obvious that the measurement of rigidity needs to be developed through the use of a variety of tasks, the conception of abstract solutions of problems as more flexible than concrete ones needs to be exploited in education.

Peters (94) has presented an interesting view of maladjustment as associated with a tendency to react on an abstract level without awareness of the tendency. Data obtained by means of a free association test of the tendency to give supra-ordinates in discrete free associations yielded evidence in support of his hypotheses. The results have interesting implications for flexibility in teaching and learning.

Richards (98) describes a case study, covering a period of over seven years, showing how a boy's IQ rose and fell twice, to a surprising extent. The rises and drops in IQ were associated with problems arising in the family and in the boy's experiences at home and school. While the data are in support of his hypothesis that mental test performance can reflect stresses and strains in life, it would seem that the normative influence of data from other cases would be needed for full interpretation of the results.

A general study of frustration in adolescent youth and its implications for the school program has been published by Segel (102). It is suggested that, in order to minimize frustrations, the curriculum should fit the child, have immediate practical values, not place emphasis upon subject matter, and not postpone rewards into the vague future. The viewpoint is that frustrations, as revealed in school leaving, disorders in class, or loss of enthusiasm, can be identified through psychological methods. Relief can then be given through activities leading to self-expression.

Such publications are representative of those now serving as the determiners of action in school guidance and instruction. They are alike in tone

and emphasis. The main fault of the exploratory studies seems to be the erection of elaborate theory upon a rather narrow experimental base. The refreshing aspect of the work is the attempt to provide a rational or theoretical basis for treatment of maladjustments among school pupils.

PUPIL PERSONNEL

Personality factors associated with status.—In recent years in educational psychology, there has been much activity concerned with self-confidence, self-adjustment, social adjustment, and status of pupils within groups. Justman & Wrightstone (70) compared three sociometric techniques for measuring pupil status in the classroom. Data from large groups of superior eighth-grade pupils indicated that the modified Moreno method and the Ohio Social Acceptance Scale may be used interchangeably. A modified form of the Guess-Who technique seemed to measure slightly different aspects of pupil status.

Tuddenham's study (118) of factors correlated with popularity among elementary school pupils dealt with coherence of item groups in reputation data. Factor analysis indicated two main clusters. The first emphasized the importance of athletic skill and dominance and appears to define the pattern most valued and most rewarded among boys. The second cluster apparently emphasized poise, balance, and maturity of behavior as adjusted to adult requirements. This cluster of traits is much more valued among girls than boys. The suggestion is made that for a boy securing group approval is dependent upon conforming to a well-defined group of traits demanding strength and motor skill, while for a girl the problem is one of adapting to a continuously changing set of values which are more complex and less clearly-defined than those of boys. The study was based upon 1439 children in 42 classrooms, ranging from grade one through grade five.

Williamson & Hoyt (128), using the MMPI, measured the personality characteristics of student leaders at the college level. The most evident differences indicated that student leaders in political activities, especially radical or liberal ones, are more neurotic and unstable than other student leaders. Fraternity and sorority leaders tend to be just characteristic groups of well-adjusted and otherwise typical students.

Mature and critical attitudes.—The studies reviewed in this section have in common a tendency to investigate the status or progress of young persons toward certain goals of adjustment to the adult world. Amatora (2) analyzed data showing that elementary school pupils are critical and discriminating when they rate their teachers. The suggestion is made that pupil reactions offer a source of easily obtained information which teachers ought to use as a basis for self-improvement.

In a study of college groups, Garrison (48) found that the more mature and advanced students were more favorable toward increasing the power of the United Nations. A majority approve extension of government control of economic and military resources and medical services. A fairly large minority

believe in continued control by private industry, or professional groups. Inlow (67), in a study of college graduates, found job satisfaction was not related to college grades, and little related to job categories. The majority of the graduates were well satisfied. Degree of satisfaction was related to time spent on the job, salary, status, age, marital status, etc. Inlow interprets the results as showing that job satisfaction is not isolated or discrete, but instead appears to be part of a general pattern of personality, interests, and experiences.

In discussing counseling in the Air Force, Borg (16) suggests that a peer rating method might offer the best solution to the problems of correct vocational placement in Air Force Careers. This conclusion is based upon evidence that the counselees themselves are the only ones having the knowledge and experience necessary for judging the fitness of each candidate for a given type of service.

INSTRUCTIONAL METHODS

Instruction in general.—The superiority of methods other than a lecture method of instruction has often been noted. For example, Levine & Butler (74) discovered that a group discussion method is more effective in producing changes in behavior. Nevertheless, as pointed out by Schueler (101), formal lecture and formal recitation methods continue in vogue because of expedience, inertia, and tradition.

The instructional problem is often one of communicating. Malter (82), in dealing with methods of indicating the true sizes of pictured objects, has provided a critical exploration of communication by means of visual aids. As shown by Grace (53), knowledge of an audience tends to change the content of communication. Studies of readiness, like that of Brownell (20), may also be interpreted in this context. Brownell's study indicated specifically a need for teachers to know more about their pupils' achievements in order to be able to introduce new topics of instruction at appropriate times.

A type of contribution which appears every year is that dealing with remedial instruction. A typical study is that by McGinnis (78), who showed that students enrolled in a reading laboratory learned more than those in a control group. Studies of this type show almost uniformly that the type of instruction dealt with experimentally is more effective than "more ordinary methods" as used in control groups. Among other things, such studies indicate that instruction tends to be better when some special attention is paid to it.

An article by Slobetz (105) shows that in dealing with problem behavior, teachers tend to search for causes and to emphasize the positive side of treatment. In particular, they tend to place special emphasis upon constructive techniques of handling withdrawing and recessive behavior. They feel more frustrated and less adequate in dealing with disturbances of classroom decorum and with aggressive sabotage of instruction. They rate aggressive conduct disorders as more annoying but less serious than quiet and withdrawn

types of problem behavior. The data indicate that currently popular psychological points of view are incorporated into the thinking of teachers in dealing with these troublesome aspects of teaching.

Teaching elementary psychology.—Burnett (22) and Marcuse (83) have offered suggestions for improvement of teaching at the college level. Burnett's study (22) showed that students would like instruction in "how to study." They would also like increased use of teaching methods involving experiment and demonstration, visual aids, and panel discussions. They recommend more emphasis upon aptitude and personality testing. While some of their suggestions, such as covering less material, eliminating technical material, relating content more to everyday life, etc., do indicate some of the limitations of students, they also serve to remind us of some of the basic problems of teaching.

The study by Marcuse (83) suggests improvement of the old lecture-recitation method of teaching through use of lecture combined with extensive discussion. The data signify that this can be effective even in large classes with enrollments between 150 and 200. Preferences of students are for smaller groups and participation in class activities is increased when groups are made smaller.

Teaching educational psychology.—Numerous reports indicate that new instructional methods are being tried out in educational psychology. The prevailing feeling is that the methods of teaching in this course should illustrate the principles taught. There are of course practical difficulties, such as budgetary limitations, which stand in the way of progress toward this ideal.

As shown in reports by Baller (6), Horrocks (63, 64), Murphy (87), and Rivlin (99), the technique currently most emphasized is that of studying a child and preparing and discussing a case-study report. Other techniques include appropriate use of ability grouping, direction of committee work by students, use of films, pupil participation in evaluation, and various types of group projects. Murphy (87) emphasizes getting acquainted with students and guiding students in observation of children. Rivlin (99) emphasizes long-continued study of an ordinary or normal child. Both Rivlin and Horrocks emphasize instruction in small groups. Project methods of instruction are discussed by Morlan (86) and by Stone (110). Morlan's study of student reactions indicated a great many cynical and unfavorable reactions toward term papers. Stone (110) recommends student participation in minor research projects as a method of instruction.

RESEARCH METHODS

A report by Conrad (27) dealt rather broadly with research in education, indicating its impact upon theory and practice and suggesting ways of making progress toward solution of recognized problems. Possibly the best indication of current methods in research is to be obtained through survey of all the studies reported in this chapter, with special consideration of the methods used. As a compromise, we may mention a few illustrative studies. In that by

Grace & Grace (54), for example, the method consisted of construction of a measuring instrument, collection of data by testing a large group of college students, and reduction of data by means of factor analysis. This procedure led to construction of rational categories of classification of behavior, with resulting clarification of results. Another illustrative study is that by Littleton (75), dealing with prediction in auto trade courses. The method consisted of application of a battery of tests and checking against a criterion of instructors' ratings in shop work. This was followed by multiple correlation analysis, showing correlations of .62 and .59 for prediction of success in auto trade courses by means of combinations of tests.

A book by Barker & Wright (7) presents a detailed record of behavior of a seven-year-old boy observed intensively for 14 hours. This type of report indicates a faith in the value of bringing students into contact with facts of behavior exposed in all their natural complexity. Perhaps it indicates a revolt against the confining consequences of making decisions such as are involved in analysis and reduction of data.

A special technique very widely used is that of item analysis. Studies like those of Ebel (39) and of Johnson (68) are in essential agreement with earlier studies in showing the relatively high degree of effectiveness of simple techniques for improvement of reliability and validity of tests.

Researchers in educational psychology continue to make extensive use of simple statistical techniques for description and inference. The simple methods of measuring central tendency, variability, and relationship are followed by more complicated analysis by multiple correlation, analysis of variance, and factor analysis. The testing of hypotheses by collection of data upon experimental and control groups, followed by measurement and tests of the significance of differences between groups, is prevalent. While most studies in the field are not strictly experimental, it is also true that uncritical acceptance of observational results is out of fashion. The modern worker in educational psychology tests his findings in the light of theory of probability.

EVALUATION

Evaluation of education.—Durost's study (38) showed that students in the secondary schools have little acquaintance with literature and appear not to be reading it. The finding raises important questions. Should young people have more contact with the best thought which has been expressed? Should one evaluate the achievement of the pupils in this area through use of tests more closely tied in with the curricular requirements of the individual school system?

In a study by Fulk & Harrell (47) scores on the Army General Classification Test for both Negroes and whites were clearly shown to be related to grade levels reached in school. The finding has descriptive significance, although of course one may recognize that it may not be entirely due to the implied educational influences.

A study by Vukota (121) indicated that the educational achievement of

bright pupils in the high schools of a large city was consistently very superior, averaging above the ninetieth percentile on nationally normed tests. The bright pupils tend to remain in school beyond high school, while fairly large numbers of others drop out in grades 10, 11, and 12. These results, along with other data, are interpreted as showing that the educational offerings of that city are well fitted to the interests and abilities of superior students.

Evaluation of specific processes.—A framework for evaluation of certain counseling procedures has been presented by Bordin (15), in a study of ethical responsibilities of instructors in testing courses. The ultimate basis for restriction of enrollment and of uses of tests is the desire to protect the pupils and the public from malpractices. A study of the uses of pupil ratings in the evaluation of teachers has been published by Cooper (28), who concluded that pupil ratings are valid and reliable indices of teaching adequacy. Incidentally, such ratings showed as usual that young student teachers are as acceptable to pupils as are the more experienced regular teachers.

Using achievement test data from students at Antioch College, Dole (35) found that students earning credits by examination retained their information at least as well as those who took regular courses. Numerous advantages were seen in the method of giving superior students opportunities to earn credits by examination. In the evaluation of techniques in teaching and rating of oral expression, Di Vesta & Roach (34) made effective use of student ratings. The items of their rating scale will interest teachers of public speaking.

Watson & Mensh (124) report a case study showing the effectiveness of psychotherapy in helping a young man troubled by social maladjustments. Further proposals for evaluation of progress in psychotherapy are presented by Watson, Mensh & Gildea (125). Evaluation of techniques for assessment of leadership is implied in the article by Bass & Norton (12), and of course to some extent in other studies of situational tests. It appears that groups of six are optimal in leaderless group discussion technique, but that much variation in size of groups can be permitted.

A study by Webb (126) revealed interesting interdepartmental differences in reliability and validity of faculty ratings of student achievement. Students in education showed lower scores on the Cooperative English Test and the Cooperative General Culture Test than did graduate students in other fields. The data led to the suggestion that selective procedures for use in teacher-training institutions need to be developed and used more effectively.

EDUCATIONAL MEASUREMENT

Differential testing.—While the interest in differential, analytic, and diagnostic testing continues, critical and questioning tendencies are increasing. Optimism is being replaced by realism. Barrett (8) found that total scores on the ACE test were best for the prediction of grades in mathematics. The fact

that Q scores were not reliably better than L scores for prediction of success in this subject area was stressed. A longitudinal study of the Differential Aptitude Tests was made by Doppelt & Bennett (36), who found evidence that scores on the tests remained fairly stable in the three years between grades 9 and 12. This, of course, does not demonstrate the diagnostic or analytic significance of those scores.

In using the tests of Primary Mental Abilities with bright five- and six-year-old children, McKee (79) found evidence that these tests are not suitable for use in longitudinal studies. The ceilings were too low for seven-year-olds and bright six-year-olds, and the tests were too long for use with younger children.

Sources of error.—The effects of various chance techniques of determining scores on the MMPI were investigated by Cottle & Powell (30). Answering most of the items "true," tended to produce a psychotic profile; answering most of them "false," tended to produce a neurotic profile. Profiles of scores determined by chance tended to deviate from the normal. The writers conclude that a profile within the normal range tends to be obtained only through the censoring and integrating effects of a normal personality.

Several investigations have dealt with the fakability of tests. Green's study (57) of applicants in police work indicated that some tests, like the Kuder Preference Record, should not be used in selection because applicants can easily answer the items so as to secure the desired pattern of scores. Other tests, like the Guilford-Martin GAMIN inventory, are more subtle and less easily faked. Mais (81) showed that a self-confidence key for the Jurgensen Classification Inventory was valid under normal conditions, but that the test could be falsified to secure high scores for self-confidence. Noll's study (91) showed that most college students are able to fake effectively to secure high scores for good adjustment on the Minnesota Personality Scale. On a less subtle instrument, the Cornell Index, faking was even easier. No relationship was found between IQ and success in faking. Women were more successful at it than were men.

Specialized test approaches.—Thompson & Powell (115) studied a rating scale technique for measurement of social status of sixth grade children. Use of the negative end of the rating scale had certain advantages and appeared to have no undesirable effects. The rating scale required each child to make a judgment as to the social acceptability of every other child in the classroom. The technique is seen as more efficient than other methods which make use of partial rank orders.

A device for measuring social class identification has been presented by Sims (104). It consists of a list of occupations to be judged as same, higher, or lower than the one in which the respondent places himself. The scale promises to be useful. Evidence is presented that it is sufficiently subtle to discriminate and to yield reliable results.

In a plea for the use of the open-book type of examination, Tussing (119) cited all the usual clichés in criticism of the ordinary examination method.

His arguments in favor of the open-book examination are interesting assertions, useful as a basis for empirical studies. In study of experimental and control groups of college students, Odom & Miles (92) found evidence that oral administration of true-false tests does not compare unfavorably with the usual administration in mimeographed form. Traxler (117) presented an account of the development of objective tests in the field of accounting. This appears to follow the trend in which various professional groups have turned to the use of objective tests for solution of measurement problems. In accounting, need is felt for measures of general intelligence, achievement in accounting, vocational interests, and personal qualities. The tests as developed appear very promising.

Readability and related variables.—The testing of readability, as presented by Flesch (46), has been the subject of much debate. Omitting purely controversial statements, we may note that Farr and his collaborators (43) introduce results of an empirical study to support their claims. They show that saving of time would result from substitution of the number of one-syllable words per 100 words in place of the usual syllable count required as part of the Flesch formula.

Various readability formulas have been presented and have been available for some time. A study by Michaelis & Tyler (85) indicates that the readability levels given for certain social studies materials by the various formulas are in marked disagreement. Using United Nations materials, they found the Flesch and Dale-Chall formulas superior to the Lorge formula when levels of readability were checked against levels of reading comprehension provided by means of reading test results. The Lorge formula yielded an underestimation of the reading difficulty of the social studies materials.

Russell & Fea (100) investigated the validity of six readability formulas as applied to juvenile fiction. For selected well-known children's books, the mean ratings of difficulty obtained from children's librarians provided a stable criterion. Checked against this criterion, the Lewerenz formula underestimated the reading difficulty of the books, and the Yoakam and Winnetka formulas erred in the opposite direction. The Dale-Chall, Flesch, and Lorge formulas appeared about equally good for rating the difficulty levels of the books. The authors see difficulty of reading material as partially dependent upon difficulty of concepts, arrangement of writing, relation to interests, and other variables which cannot be adequately assessed through study of the difficulty level or familiarity of words taken as separate units. The studies by Michaelis & Tyler (85) and by Russell & Fea (100) are especially valuable in that they go beyond consideration of the mechanical aspects of the formulas and provide critical insight into the functioning of the available measures.

Situational tests.—The leaderless group discussion technique has been given much attention during the past year. The topic seems especially important in education, in relation to student leadership, counseling and guidance, and newer methods of instruction. Studies by Ansbacher (4) and by

Bass (10, 11) have indicated the validity and reliability of the leaderless group discussion method. Reports by Bass (11) and by Bass & White (13) have paid attention to the processes, showing in some detail the sort of question in the mind of the observer which can be answered through the leaderless discussion technique. At present, it seems likely that this method will have important educational applications.

EDUCATIONAL GUIDANCE

Problem behavior.—In a study of 1000 child guidance clinic patients, Ellis & Beechley (41) found that those coming from large families were older, less intelligent, and more retarded in school than the others. They were also less emotionally disturbed and more often from broken homes. Holding intelligence constant, those from large families were not more inclined to do poor work in school.

Nolan (90) surveyed data from 29 junior and senior high schools and found evidence that a certain pattern of school situations did contribute to improvement in cases of pre-delinquency. Beneficial effects resulted from use of suitable cumulative records, a testing program, and a program involving case studies with home visits and individual counseling. On the other hand, enrollment of problem pupils in courses of study, or treatment, in the absence of suitable guidance data seemed definitely ineffective.

Follow-up studies.—In a follow-up study of high school graduates, Latham (73) found no correlation between job success and selection of jobs appropriate to measured aptitudes. However, satisfaction was related to the taking of a job for which the individual had planned. The study suggests a need for use of the results of aptitude testing at the time when plans are being made. A limitation of the study is the fact that it dealt only with a one-year follow-up after high school. Different aspects of satisfaction and success might emerge later. Gurr (58) has reported on a follow-up study of graduates of a California high school. In the light of reactions of the graduates and their employers, suggestions were made for changing the curriculum. The approach seems realistic and one may note with satisfaction that the study is to be continued for two additional years at least.

The studies of Bailey & Brammer (5) and of Silvey (103) have shown that intelligence test scores tend to rise when pupils are re-tested after two or three years of attendance in college. There are some exceptions; some people show a drop in scores, others make astonishingly large gains. Ranks tend to remain the same, as shown by correlations varying from .60 to .80. Silvey (103) also showed that interests tend to remain stable, although with increased time spent in college, interests in clerical activities decreased, while interests in social service activities increased markedly. Changes in intelligence test scores which might indicate the more stimulating character of some curricular fields were not emphasized.

A study of citizenship attitudes of college graduates and high school graduates was conducted by Drucker & Remmers (37), showing that a gen-

eral education curriculum has better influence on citizenship attitudes than does a more technical curriculum.

A report by Strong (111) indicates surprisingly good agreement between interest test scores obtained during college and occupations followed 20 years later. In addition to the evidence of validity of the interest scale, the study provides significant suggestions as to technique for the use of such tests in guidance.

Types of guidance.—A study by Berdie (14) deals with counselor attitudes and their effects upon interviews. The article contains the suggestion that nondirective attitude is more important than nondirective technique. A number of suggestions are made concerning subtle ways in which attitudes can affect the counseling interview. Carlson & Vandever (23) used counselee ratings and evidence from the TAT in judging the effectiveness of aid given on problems of vocational choice. The data showed that persons who come voluntarily for counseling include many who react somewhat more favorably to directive than to nondirective counseling.

An article by Glover (50) dealt with the counseling activities of the teacher of courses in marriage and family relations. The pupils in such courses seek help from their teachers on a variety of problems. The teachers generally felt that individual counseling is one of the responsibilities of the teacher in such a course. A study by Golden (51) presented some evidence that high school faculty committees work more effectively when structured on the basis of sociometric data than when determined purely by administrative appointment. The appointed committee showed more work-centered behavior and less democratically organized patterns of participation in committee work.

TEACHER PERSONNEL

Using data from the Research Division of the National Educational Association, Davis (33) reported upon the amount of work required of teachers. The typical working week for teachers includes about eight hours more service than that which prevails generally among other groups of employees. The requirements are regarded as excessive, especially in view of the strain involved in the duties of a teacher. Using a projective test, Alexander (1) showed that teachers who did not express affection in dealing with children were characterized by anxiety, fatigue, and feelings of dependency. They appeared to suffer from internal conflict which prevented attack upon or solution of problems.

Recognition of a need for clarification of the social and professional roles of teachers is implicit in studies of in-service training. Newcomer (89) has described an informal method of in-service training in which principal and teachers met bi-weekly in private homes for discussion. McNassor (80) reported upon experiences of teachers after they attended a summer workshop in order to learn how to teach better and how to understand interpersonal relationships among pupils. Upon returning to their jobs, the teachers were

disappointed to find that their new techniques were not received with enthusiasm nor given support by local authorities.

Valenti (120) contrasted the formal and impersonal approach with a more informal and group-centered approach in dealing with problems of interpersonal relationships. Younger teachers were found more often in the personal, informal, and integrative role, with the older teachers tending to remain more formal and impersonal. In a study reported by Wandt (123), scales were developed to measure attitudes of teachers toward groups of persons and toward teaching procedures. Factor analysis indicated three clusters of attitudes: toward subordinates, toward peers, and toward superiors. Elementary school teachers had more favorable attitudes than secondary school teachers. Attitudes were relatively independent of years of schooling.

Continuation of studies like these, dealing with teachers as human beings, should help to solve some of their problems of social and personal relationships and should aid in clarifying their social and professional roles.

SUMMARY

Educational psychology, as presented in this review, is concerned with scientific analysis of a very broad array of educational forces. It has extended the range of application of objective and quantitative methods. Critics from outside the field express resentment at this progress in freeing evaluation in education from dominance by opinion and speculation. Critics from within note that the work is not systematized; instead, it is characterized by somewhat unco-ordinated efforts to measure and investigate all aspects of an astonishingly complex field of behavior. Too frequently for comfort it is necessary to point out that findings are tentative, rather than conclusive. The many fragmentary contributions are acceptable as sources of insight, but a need is felt for isolation of crucial problems and persistent research of a more penetrating and comprehensive character.

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STATISTICAL THEORY AND RESEARCH DESIGN¹

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INTRODUCTION

During 1951 to 1952 there has been a continuation of the trend to present in nonmathematical language statistical material not so long ago available only to readers willing and able to cope with strong doses of mathematics. These expository articles state the assumptions underlying the statistical models they discuss, and sometimes offer alternative procedures with differing assumptions for solving the same problem. Some articles present examples worked in detail, so readers are supplied with a program for computation. Another kind of expository article was noted frequently: a discussion of a widely-used statistical technique with implications that are generally misunderstood, or with a large body of "lore" having little basis in statistical fact. The main function of this chapter is to focus attention on developments in statistical theory which seem to offer some immediate utility for psychologists. The summary of theoretical advances has to be limited by space requirements, and it is hoped that the more important developments will become the topics of future expository articles. In particular, decision theory has had little in the way of exposition, though the theory is advancing rapidly.

Applied Statistics, a journal of the Royal Statistical Society, appeared for the first time in March, 1952. The editors hope to address it at least partly to social scientists. To quote from the Foreword of the first issue: "Its aim . . . is to present . . . simply and clearly, the statistical approach and its value, and to illustrate in original articles modern statistical methods in their everyday applications."

Kemphorne's new book, *The Design and Analysis of Experiments* (50), is somewhat more symbolful than the related book of Cochran & Cox (17), gives more detail on some kinds of problems, and explores the rationale in a more formal way.

The new *Educational Measurement* (55), edited by Lindquist, is designed for advanced training of measurement workers and is both a handbook and a textbook on the theory and technique of educational measurement.

Dwyer's excellent book (29) on computational methods involved in solving simultaneous linear equations is a boon to all who are plagued with nasty real-life computation. The book is particularly valuable because examples are worked in detail and layouts for specific problems are given in very clear form.

The *Journal of Clinical Psychology* used most of the January, 1952 issue

¹ The survey of the literature to which this review pertains was completed about May, 1952.

to publish a symposium entitled "Research Design in Clinical Psychology." The many authors took this opportunity to give opinions on the general principles of research design in clinical psychology, as well as arguments about the usefulness of specific research tools.

TEST METHODS

Extensive factor analyses of many kinds of test data have appeared during 1951 to 1952. The *British Journal of Psychology, Statistical Section* for 1951 contains a number of articles giving detailed results for specific factor analyses, and a lengthy article by Burt (11) discusses some of the special problems that beset test constructors as well as factor analysts. Eysenck (34) emphasizes and illustrates the uses of factor analyses: to suggest hypotheses and to test hypotheses. He calls attention to the lack of suitable criteria for significance—this lack results in chance fluctuation serving as the basis for far-reaching conclusions. He makes a plea for help in developing suitable criteria for significance. (From a mathematical point of view, the difficulty of testing significance stems from certain useful but arbitrary practices in factor analysis work.) McNemar (61) presents a somewhat related article, an amusing pseudo-factor analysis of the behavior of factor analysts in 73 unnamed published papers. To quote (61, p. 358):

... the factors in factoring behavior have to do with nabbing a small sample, ignoring other crucial sampling matters, treating the rotational problem irrationally, using tests of unknown unreliability, violating the requirement of experimentally independent measurements, predestinating the outcome, tossing in too much or not enough, choosing and ignoring tests when naming factors, struggling to make sense out of the results, and varying all over the map in the use of hypotheses.

Factor analysis, because of its technical nature, would require more space than this review can devote, and so detailed results will be omitted.

Reliability is one of the problems that continues to plague testers. An often unrecognized part of the difficulty is that there are many kinds of reliability. By improved definitions, Cronbach (19) helps clarify the issues. He investigates at length one kind of reliability coefficient—the coefficient of equivalence, which is the correlation between two forms of a test given virtually at the same time (this coefficient shows how nearly two measures of the same general trait agree). Cronbach denotes the Kuder-Richardson Formula 20 by the letter alpha and studies the behavior of alpha both theoretically and empirically. Two recommended formulas [Guttman (42a); Rulon (75a)] for alpha are

$$\alpha = 2 \left[1 - \frac{\sigma_a^2 + \sigma_b^2}{\sigma_t^2} \right] = 1 - \frac{\sigma_d^2}{\sigma_t^2},$$

where a and b are half-test scores, $t = a + b$, and $d = a - b$. The Spearman-Brown formula is a special case of alpha when the half-test scores have equal standard deviations. The Spearman-Brown formula turns out to be close to

alpha when the standard deviations of the half tests are within 10 per cent of one another. Alpha can be interpreted (a) as the mean correlation between all possible split-half tests derived from the same tests, or (b) as the expected value of the correlation when scores from two random samples of items (drawn from a pool made of items like those in the given test) are correlated. Unless tests contain large blocks of items representing group factors, split halves carefully selected to be equivalent give little larger correlation than random splits.

In an article on another facet of the reliability problem, Cronbach & Warrington (21) show that split-half reliability coefficients on speeded tests may be misleadingly high. For example, a 600-sec. spatial test gave .86 as the reliability when it was actually .54. The authors state that split-half reliability measures are acceptable if (a) the variation in number of items finished by subjects is small, or if (b) when time is called, subjects have completed all items which they have an appreciable chance of getting right (or when a correction formula is used, subjects have completed all items where probability of success is greater than chance), or if (c) fluctuations in number finished from trial to trial are small compared to fluctuation in number finished from person to person. Lower bounds are suggested for the true reliability based on single trial split-half methods. Degree of speeding can be measured if two equivalent forms A and B of a test are available by tau

$$\tau = 1 - \frac{r[\text{A timed, B power}]r[\text{A power, B timed}]}{r[\text{A timed, B timed}]r[\text{A power, B power}]},$$

where r means correlation, and "timed" and "power" refer to limited and unlimited times of administration, respectively. When tau is .90, 90 per cent of the true score variance represents speed factor, and 10 per cent represents altitude factor. In the spatial test mentioned above, tau varies from .42 to 0 as time lengthens from 600 to 4800 sec.

Horst (47) provides a method for estimating the total test reliability from parts of a test of unequal length. The reliability is

$$R = \frac{r(\sqrt{r^2 + 4pq(1-r^2)} - r)}{2pq(1-r^2)}$$

where p and q = proportions of total testing time taken by each part, $p+q=1$, r = correlation between the two parts, and R = estimated reliability of total test. The Spearman-Brown split-half reliability formula is a special case of the method given above when $p=q=\frac{1}{2}$. Horst gives a detailed example of the procedure illustrating the computation. The article has typographical errors.

A time-saving method for estimating the phi-coefficient for many items for an entire criterion group from a selected part of the total data is presented by Perry & Michael (74). An approximation is derived for estimating the phi coefficient computed from the use of "high" and "low" groups that

consist of equal proportions (e.g., upper 20 per cent and lower 20 per cent). Their estimate is

$$\phi = \frac{1}{1.253} \frac{p'}{y_{p'}} \phi_{p'}$$

where the four-fold table is given in Table I. The criterion scores are assumed

TABLE I
FOUR-FOLD TABLE FOR ESTIMATING PHI COEFFICIENT

Item Outcome	Group		Proportions
	Low	High	
Pass	b	a	$p = \frac{a+b}{n}$
Fail	d	c	$q = \frac{c+d}{n}$
Proportions	$q' = p' = \frac{a+c}{N} = \frac{b+d}{N}$		

$$n = a + b + c + d$$

N = Total sample size before removing middle group

$$p' = \frac{a-b}{n} \cdot \frac{1}{\sqrt{pq}}$$

$y_{p'}$ = Ordinate of unit normal distribution at the point of dichotomy breaking the normal so that the proportion p' lies to the right of the point.

to be normally distributed. An empirical check shows that the approximation gives fairly good agreement with phi computed from all the data, for correlation less than .60 and values of p and q between .20 and .80.

Bryan, Burke & Stewart (8) have investigated the question of whether the correction for guessing should be employed in scoring pretests. Obviously, different people may fall in the upper and lower criterion groups if a correction is employed from those who would fall in the groups when no correction is employed. A hardy testee could achieve a considerable bonus for himself, if the test is long and he guesses all the answers he doesn't know and no correction is made for guessing. Bryan's investigation shows that when tests are easy, it makes little difference whether scores are corrected for guessing or not before computing the validity indices for items. On the other hand, if the test is difficult, people afraid to guess wind up in the low criterion group when they should fall in the middle or above; people eager to guess may wind up in the high criterion group spuriously. The authors suggest that all tests be corrected for guessing before computing item statistics, be-

cause there is no way to know whether tests are difficult or easy at the outset. Many people not concerned with item analysis but concerned with making tests for the classroom will be interested in this advice.

In addition, the following articles will interest test constructors but space does not permit detailed review: Horst (48), Guilford & Perry (41), Gulliksen (42), Goheen & Davidoff (39).

SELECTION PROCEDURES

Cochran (15) states the problem of selection as follows:

One of the principal techniques for improving quality is to select those members of a population that appear to be of high quality and to reject those that appear to be of low quality. Usually the selection is based on a number of measurements that have been made on the available candidates. . . . A common feature in most selection problems is that at the time of selection we cannot measure directly the quantity which we wish to improve. . . . Stated mathematically, the problem is to improve some quantity y by means of indirect selection that is made from a group of tests or measurements

Cochran presents the principal known results for problems in selection and discusses practical applications. In direct selection, if y is the score on the criterion, we choose the best proportion α of people, and the gain from using the y -test is

$$\frac{z(\alpha)}{\alpha}.$$

For a normal distribution of y 's, α is the proportion to the right of the cut-off point, and $z(\alpha)$ is the ordinate at the cut-off point. Of course, in many practical situations, to get a direct measure, y , is not feasible. This leads to indirect selection, using some test x that correlates with y . Now the gain by indirect selection is some fraction of the gain by direct selection, and in fact is

$$\text{gain by indirect selection} = R \times \text{gain by direct selection}$$

where R is the multiple correlation between y and the x 's (providing y and the x 's are multivariately normally distributed).

Cochran also discusses the problem of two stages of selection, and gives some examples. Two stage selection may decrease costs if one can first use an inexpensive battery to weed out most candidates, and then use a second more searching and perhaps more expensive test for choosing among the survivors of the first battery.

A special problem in selection arises when it is desired to select for more than one objective; for example, several different types of jobs need to be filled using the results of a single test battery. Brogden (7) shows that the use of differential predictors (a battery of tests with differential weighting for each type of job to be filled) will provide substantial increases in efficiency over the efficiency of a single predictor (a single composite measure having

the highest average validity for all jobs). Three factors influence the efficiency of differential predictors: (a) the variation in the proportion of total applicants to be assigned to jobs, (b) the number of different jobs to be filled (the cases of one to four jobs are treated), (c) the degree of correlation between the differential predictors. Brogden presents tables of the value of the criterion variable showing the influence of the selection ratio, the number of jobs, and varying amounts of intercorrelation in the differential predictors. Details of mathematics and arithmetic are given in an appendix to the paper. An interesting result for two differential predictors, each with validity .50, is the following: As the degree of correlation between differential predictors becomes larger, as one would expect, the gains from using differential predictors become smaller; however, approximately one-half the gain from using differential predictors is still obtained when the correlation is .80.

Mollenkopf (65) concentrates on a different aspect of the problem of differential prediction: How does prediction efficiency vary in relation to the validity patterns of two predictors? (All test validities were assumed to be .50 in Brogden's examples.) Mollenkopf's problem is also different in that all individuals are candidates for assignment, and each will be placed in one of two categories. He provides a chart showing the losses in differential prediction with increasing predicted-score intercorrelations. Mollenkopf examines results using two and three tests with differing validities, including .00, .10, .30, and .50 in many combinations. An interesting extension of the problem is this: In addition to finding the likelihood of doing better in activity *a* than in activity *b*, we may also be interested in a good estimate of how well an individual will do in each activity. For this problem, unlike the first problem, the inclusion of a generally valid test improves the prediction.

NONPARAMETRIC STATISTICS

The number of techniques which do not depend on assumptions about the specific nature of the distribution function from which data are drawn is steadily growing. A fine review article by Moses (67) describes the application of some of the most useful nonparametric methods to psychological data. Moses' article lists the assumptions that underlie the methods he outlines, and also provides worked examples. The number of topics covered is too large to permit detailed review here. Moses discusses the sign test, the median test, the Wilcoxon matched pair test, and signed ranks. He also discusses the Mann-Whitney U test (for unmatched groups), and analysis of variance for ranked data. The tests described, besides being distribution-free, are usually much easier and less time-consuming to compute than their parametric siblings. Most psychologists will find these tests a valuable supplement to their parametric statistical methods, and the lucid explanation in Moses' article should encourage widespread use.

Blomqvist (4) has invented a nonparametric method to apply to the following problem: An experimenter has measured subjects on a number of different characteristics and is interested in testing whether the variables seem

to "go together" as a set, i.e., whether high values on one variable go with high values on another variable. The test for this "relatedness" can be done quickly as follows.

TABLE II
ORIGINAL DATA AND DICHOTOMIZED DATA FOR
BLOMQUIST'S "RELATEDNESS" PROBLEM

Original Data					Dichotomized Data					Totals
Subject	Variable				Subject	Variable				
	1	2	3	4		1	2	3	4	
1	835	4	+	.05	1	1	1	1	0	3 = y_1
2	241	4	++	.21	2	1	1	1	1	4 = y_2
3	96	1	—	.01	3	0	1	0	0	1 = y_3
4	20	0	--	.64	4	0	0	0	1	1 = y_4
5	174	2	+	.89	5	1	1	1	1	4 = y_5
6	35	0	—	.22	6	0	0	0	1	1 = y_6
7	99	0	—	.10	7	1	0	0	0	1 = y_7
8	10	0	+	.02	8	0	0	1	0	1 = y_8
Totals					4	4	4	4		
					n_1	n_2	n_3	n_4		

The experimenter needs to decide which end of the scale is "high" on each variable. In Table II he decides for variables 1 through 4 respectively: 835 is high; 4 is high; plus-plus is high; .89 is high. He decides the order for variable 3 is plus-plus, plus, minus, minus-minus. First dichotomize each variable and assign 1 to high values, 0 to low values (see "Dichotomized Data" in Table II). Our example applies to the case where each variable can be given half 1's and half 0's, otherwise reference to the original article is essential. Splits of about half high and half low are best.

If we have randomness instead of correlation from column to column we expect a row total of 1's and 0's to be

$$P = \frac{1}{n} [n_1 + n_2 + \dots + n_m]$$

where n is the number of subjects, and n_1, n_2, \dots, n_m are the numbers of 1's in the m columns. For 50-50 splits, $P = m/2$. If we have high mutually positive correlation, then the sum of squares

$$S = (y_1 - P)^2 + (y_2 - P)^2 + \dots + (y_n - P)^2 = \sum y_i^2 - nP^2$$

should be large. Here the y 's are the row totals of the dichotomized data (see Table II, column labelled "Totals" under Dichotomized Data). There are three main tests proposed corresponding to the cases: (a) a few variables but a large number of subjects, (b) a few subjects but a large number of variables, and (c) few subjects and few variables. The tests are:

(a) With few variables but many subjects, the sum of squares S is nearly normal with mean $mn/4$ and variance $n^2m(m-1)/8(n-1)$, providing 50-50 splits are applied to all variables.

(b) With few subjects but many variables the quantity

$$\frac{4(n-1)S}{mn}$$

is distributed approximately like chi square with $n-1$ degrees of freedom, when the variables are dichotomized 50-50.

(c) For small numbers of both subjects and variables, tables of percentage points of S are given.

For the data of the example, $m=4$, and $n=8$, the third method is appropriate, but we deliberately illustrate all three methods.:

Method (a)

$$P = m/2 = 4/2 = 2$$

$$S = 3^2 + 4^2 + 1^2 + 1^2 + 4^2 + 1^2 + 1^2 + 1^2 - 8(2)^2 = 46 - 32 = 14$$

$$\text{average } (S) = mn/4 = 32/4 = 8 \quad \text{variance } (S) = 8^2 4(3)/8(7) = 96/7 = 13.72$$

$$\text{standard deviation } (S) = 3.70$$

$$t = \frac{|S - \text{average } (S)| - 1}{\text{standard deviation } (S)} = \frac{|14 - 8| - 1}{3.70} = \frac{5}{3.70} = 1.35. \quad P(t > 1.35) = 0.089.$$

We use a one-sided test because the alternative hypothesis is positive correlation. The sign of $S - \text{average } (S)$ is taken as positive, as indicated by the vertical bars. The minus one in the numerator of t is a correction for continuity.

Method (b)

$$\frac{4(n-1)S}{mn} = \frac{4 \times 7 \times 14}{4 \times 8} = 12.25; \quad P(\chi^2 > 12.25) = 0.093.$$

Method (c). Looking up the value of S for $n=8$, $m=4$ in Blomqvist's tables gives

$$P(S \geq 14) = 0.0912.$$

The test is fairly easy to apply even to large tables.

RANKING METHODS

A number of further articles on ranking methods, some giving new methods and others refining older methods, became available in 1951 to 1952. These nonparametric procedures have been extensively used by psychologists in the past and the new developments noted here provide solutions for further special types of problems. Practically everyone uses the rank correlation coefficient, especially in cases where it is desired to get a quick estimate of correlation. When the number of pairs of observations gets to be large, however, the work needed to rank all the observations may be excessive, so that the saving in time over computing the product-moment correlation coefficient becomes negligible. A new correlation coefficient of the ranking type is

described by Chown & Moran (14), and it estimates the usual correlation coefficient with about one-third efficiency, when the true correlation is near zero, or in other words, preserves one-third of the original information available before the observations were ranked. This appears to be a practical and useful technique when the number of observations is large. They provide also a similar technique for making quick estimates of serial correlation. For

TABLE III

CHOWN & MORAN'S METHOD OF ESTIMATING THE CORRELATION COEFFICIENT

x	Coded Differences	y	Coded Differences	Products
5		2		
	1		1	1
2		0		
	-1		-1	1
4		5		
	-1		1	-1
5		4		
	1		1	1
4		1		
	1		0	0
3		1		
	0		0	0
3		1		
	1		-1	-1
2		4		
	1		1	1
1		1		
	-1		-1	1
5		5		
	-1		1	-1
6		3		
				—
				Sum 2
				$g = 2/10 = 0.2$

Estimate of correlation = $\sin [90^\circ(0.2)] = \sin 18^\circ = .309$.

estimating the correlation coefficient the x, y pairs must be in random order vertically. The steps are: (a) between each pair of x 's place 1, 0, or -1 according as the x higher on the sheet is greater, equal, or less than the one below it, (b) do the same for the y 's, (c) compute the sum of the products of the derived numbers appearing on the same line and divide by the number of products, calling this average g , (d) $\sin[90^\circ g]$ is the estimate of the correlation. In the example shown in Table III, the sample is too small for use of the method.

The product moment correlation is .487, illustrating that the rapid method cannot be expected to be close to the product moment result when samples are small.

Work is proceeding on complicated problems concerned with ranked data, such as special types of analysis of variance problems. These will be discussed only briefly, to give interested readers an idea of the kinds of problems attacked and the ease or laboriousness of the methods proposed. Ehrenburg (31) emphasizes again the fallacy often overlooked by users of ranking methods: normally-transformed ranks are not a random sample from a normal population even though the ranks are randomly assigned. He gives an easy modification of an F test for use with analysis of variance on ranked data. Moses (67), mentioned above, also reviews analysis of variance procedures with ranked data. Stuart (78) gives a method to test whether all the means are equal in analysis of variance on ranked data (the usual analysis of variance test) and then shows how it is possible to test more interesting alternatives requiring estimates of individual variances and covariances. It should be remarked that Stuart's procedure is extremely complicated and laborious, and involves such things as fitting a Pearson frequency curve by the method of moments.

A rather special problem, but a useful one from the viewpoint of psychology, is the use of incomplete blocks in ranking experiments described by Durbin (27). The problem is to rank a large number of objects when each judge ranks only a fraction of the total list. There are many psychological problems—rating occupations for instance—where the list of objects is too long for single individuals to complete a full ranking (from fatigue if nothing else). It is shown that a suitable measure to test the hypothesis of independence among the rankings is a transformation of the W statistic [Kendall (51, p. 420)]. The computations necessary to carry out this analysis are fairly simple and not very laborious. Durbin provides an example worked out in detail to illustrate the computational procedure and the procedure of making the test.

The exact distribution for the rank correlation coefficient for both 9 and 10 rankings (when the ranked variables are independent) became available this year [David, Kendall & Stuart (24)]. Heretofore distributions were available through eight rankings, e.g. Kendall (51, pp. 396–97), and 10 seems to reach the upper limit of computational feasibility. The fact of the matter is that high speed computers do not take to operations whose numbers increase factorially much better than human beings do.

A new way of attacking the ordering problem using a bivariate extension of the Mann-Whitney U statistic [see for example Moses (67)] is proposed by Whitney (83). In a sense, this can be regarded as a ranking method. Suppose we have three experimental conditions A, B, C, for subjects, and different subjects are tested under each condition. We might like to test whether Condition A yields higher scores than B, and whether B gives higher scores than C (these orders must be specified in advance of the experiment, not

after peeking at the data). A slightly different hypothesis is that A scores exceed both B and C scores. Whitney has invented a relatively easy test of these hypotheses which will not be given here. When suitable tables become available, the test will boil down to counting the number of times subjects in Condition B exceed subjects in Condition A, and the number of times subjects in Condition C exceed subjects in Condition A, and consulting a table. Meanwhile after a little calculation, it is necessary to refer to Pearson's tables (73) of the bivariate normal with correlation.

CONTINGENCY TABLES

Most of us are aware that Fisher provided an "exact test" for association in the 2×2 table. We are also aware that this exact method is tedious, and that in its stead the easier chi square test can be used as a good approximation, especially if Yates' correction for continuity is applied. Adler (1) reproachfully documents the difficulties encountered by practitioners wishing to know just how to use the chi square approximation. For instance, many authors including Yates do not tell clearly how to make the correction; no author makes a clear statement about how to handle one-sided as opposed to two-sided alternatives. In addition to these problems that have been settled but not very well communicated, there are the unsettled problems. These are: What reasonable conditions with regard to asymmetry, cell frequencies, expected cell frequencies, and total frequencies need to be fulfilled in order for the chi square approximation to be appropriate? Adler's paper makes instructive and amusing reading.

Leslie (54) has a new fast way of computing chi square for contingency tables, especially useful when both number of rows and number of columns is larger than two. Nevertheless, we will illustrate with a 2×3 in Table IV.

TABLE IV
LESLIE'S METHOD FOR COMPUTING CHI SQUARE IN
LARGE CONTINGENCY TABLES

Original Table					Computation Table				
Columns				Totals	Columns				Totals
Rows	5	10	35	50	Rows	25	100	1225	0.02
	15	40	45	100		225	1600	2025	0.01
Totals	20	50	80	150		0.05	0.02	0.0125	

$$\begin{aligned}
 \chi^2 &= 150[0.02(0.05 \times 25 + 0.02 \times 100 + 0.0125 \times 1225) \\
 &\quad + 0.01(0.05 \times 225 + 0.02 \times 1600 + 0.0125 \times 2025) - 1] \\
 &= 150[0.02(18.5625) + 0.01(68.5625) - 1] \\
 &= 150(0.056875) \\
 &= 8.53125
 \end{aligned}$$

Beside the original table, write a new table replacing the observations by their squares, and the row and column sums by their reciprocals (use table of squares and reciprocals). Now multiply "cell squared" by its "row reciprocal" and its "column reciprocal" for all cells and sum these products. Subtract one from this total and multiply the result by the total sample size. This is chi square. This procedure eliminates five subtractions, and the squaring and division operations are introduced at a time when they may be looked up in tables. Because most subtractions are eliminated, this procedure is a little handier for slide-rule users too.

Fisher's exact test has been available for a long time, and is especially useful for testing 2×2 tables with small numbers in the cells, although it is some bother to compute. Freeman & Halton (36) provide an exact test for an $r \times s \times k$ table, so now one can make an exact test on a three-way table with more than one cut on each variable. The procedure is exceedingly laborious, as an inspection of their examples for $2 \times 2 \times 2$ and $2 \times 3 \times 3$ will show. As in Fisher's test, it is necessary to write out other possible tables with the same margins, but now the possibilities are greatly increased in number. In the $2 \times 2 \times 2$ example given, the authors need to write out 43 possible tables, although they do provide a short-cut to save on calculations involving factorials. The procedure is too lengthy to display here. This treatment for contingency tables is probably too long and hard for most contingency tables for it to be of use very often, but it is easy to imagine situations where observations are very expensive and/or hard to come by, and with small numbers in the final table—in these cases such an analysis might be feasible or even advisable.

REGRESSION

Wallis (81) writing on tolerance intervals for linear regression says:

Elementary textbooks frequently give the impression that lines drawn parallel to a least squares linear regression at a distance, measured in the direction of the dependent variable, equal to the standard error of estimate will include about 68 per cent of future observations from the same population, that lines at a distance equal to three times the standard error of estimate will include 99.7 per cent, and so forth.

Anyone who has this textbook impression is mistaken about various fine points of statistical method and inference, as well as major philosophical points. One difficulty is that we can never be certain that the given future percentage will be contained between the lines. Second, when we attempt to construct appropriate parallel (rather than curved) lines we run into computational difficulties. The nearest present-day statistics comes to providing a statement like the quotation is to set tolerance limits, i.e., set up curves about the observed regression line such that we have a given confidence (not certainty) that a fixed percentage of the population is contained between the curves. Wallis explains how to set up such curves when y is a normally

distributed random variable with mean a linear function of a second variable x and where the standard deviation of y is the same for all values of x . He reviews the literature and provides a worked computational example. It is important not to assume hastily that this problem is identical with the problem of setting confidence limits on a regression line.

A regression problem of a different sort is treated by Stevens (76) in fitting the three-parameter exponential

$$y = \alpha + \beta p^x.$$

Although curve-fitting is not now the great indoor sport it once was, there comes a time when one might like to fit this exponential to some kind of growth or learning curve. In the past good fitting required great labor. Stevens provides a new, more efficient fitting method and tables that greatly facilitate the work. In order to use the tables, it is necessary to have five, six, or 7 equally-spaced values of x .

A special computing method has been proposed by Dwyer (29) for working regression and multiple correlation problems. It is called "the square root method." The method has much to recommend it: compactness, speed, accuracy. Short orderly programs for choosing the "best" small set of predictor variables among a large set are hard to come by. Summerfield & Lubin (79) using the square root technique develop a program for choosing a good small set of variables. In the illustration, a 10-test battery is replaced by a two-test battery, with apparently little loss of predictive power.

QUICK METHODS

A very useful short-cut has been suggested by Mantel (57) for estimating the standard deviation of the mean in small samples:

$$\text{estimate of } \sigma_{\bar{x}} = \frac{\text{largest observation} - \text{smallest observation}}{\text{number of observations in sample}}.$$

For samples of less than 15, this method will give estimates of the standard deviation of the mean with a high level of accuracy and only a small bias. An example will show the closeness of an estimate. With the six observations 6, 7, 10, 12, 15, 18, Mantel's procedure gives:

$$\frac{18 - 6}{6} = \frac{12}{6} = 2.00.$$

If we had used the usual formula, we would have

$$\sqrt{\frac{\sum(x_i - \bar{x})^2}{n(n-1)}} = \sqrt{\frac{878 - 768}{30}} = \sqrt{3.67} = 1.92.$$

The estimate 2.00 is very close indeed to 1.92, and if the reader cares to check further examples he may be surprised to find the estimate consistently close. Further, the approximation seems to be good for a wide variety of distributions, and holds fairly well for even a rectangular distribution up to samples of size 10. The utility of this method can hardly be over-empha-

sized for hand calculation, especially for samples involving observations whose values are larger than 20, for most persons do not know off-hand the squares of numbers larger than 20. For instance, consider a sample Mantel gives: 33, 45, 49, 51, 51, 52, 54, 57. We need only compute $57 - 33 = 24$, $24/8 = 3.00$ (estimate), whereas to get the sum of squares necessary for the standard calculation we would need at least a table of squares and probably would prefer a calculator, and to complete the computation we would have to perform a subtraction, a multiplication, a division, and take a square root.

A t test, involving even a little less computation than a t test using Mantel's method for estimating the standard deviation of the mean, has been extensively investigated by Pillai (75). Lord (56) earlier provided tables of the significance levels of a statistic G ,

$$G = \frac{\text{sample mean} - \text{hypothetical mean}}{\text{largest observation} - \text{smallest observation}}$$

for samples of sizes 2 to 20. Pillai compares the average lengths of confidence intervals derived from t and G . For 5 per cent levels ($n = 2$ to 20) the average length from t is at most 2 per cent shorter than that for G , while at the 1 per cent level the average length from t is at most 3.5 per cent shorter than that from G . In other words, the G test is nearly as efficient as the t test for small samples. Persons who are weary of computing t will want to investigate G .

Moshman (68) suggests an easy method of testing for an extreme row or column mean in a two-way analysis of variance. Suppose you observe that one of the treatment means in an analysis of variance is much greater than the others. You ask: Can I be confident that this seemingly best treatment is superior to the other treatments? Moshman provides a way of answering this question, using range estimates of variability rather than the usual sums-of-squares approach, so much time is saved. He gives tables for testing an extreme marginal mean in a two-way classification of variance. Examples are provided.

This kind of question—is this treatment better than the others—and its answer are beginning to be tackled both through analysis of variance and as mentioned earlier, through nonparametric methods [Whitney (83)]. It is fair to hope that we will soon see the day when the analysis of variance yields answers to the interesting ordering questions. The standard uninteresting question is: Is it reasonable that all subjects have the same population means? Experimenters do not often care about the answer to this question; indeed, usually the design of the experiment is such that it is almost guaranteed from the beginning that the answer to this question is "no." Tukey (80) and Duncan (25) have provided some new questions to ask of analysis of variance, and some ways of answering them. New and better methods of handling this ordering problem will undoubtedly be coming along, and psychologists will want to be on the alert for these new methods. Quite a lot of work will have to be done before the psychological profession can settle on any one standard procedure, so the treatment of the ordering problem may be a little chaotic through the next few years.

EFFECTS OF SOME COMMON STATISTICS OF DEPARTURES FROM ASSUMPTIONS

To test the significance of the difference between means of two independent groups, most of us use the well-known form of the t test given by

$$t = (\bar{x}_1 - \bar{x}_2) / \sqrt{\frac{(n_1 - 1)s_1^2 + (n_2 - 1)s_2^2}{n_1 + n_2 - 2} \left(\frac{1}{n_1} + \frac{1}{n_2} \right)},$$

where the barred x 's are sample means, the n 's are sample sizes, and the s^2 's are unbiased estimates of variance (sum of squares of deviations from sample mean divided by $n-1$). Many do not realize that this test (like its more general form, the analysis of variance) assumes not only normality of the x 's, but also that the true variance associated with each test group is the same. In any practical situation, the variance associated with each of two groups may or may not be the same, and further, usually we cannot discover the true magnitude of the ratio of true variances when it is not unity. An alternative test usually recommended for use only with large samples is

$$v = \frac{\bar{x}_1 - \bar{x}_2}{\sqrt{\frac{s_1^2}{n_1} + \frac{s_2^2}{n_2}}}.$$

When $n_1 = n_2$, t and v are identical. The significance of both t and v should be looked up in a t table with $n_1 + n_2 - 2$ degrees of freedom. Gronow (40) has carried out some calculations (with 18 degrees of freedom) to try to see what kinds of departures from the assumption of equal variances have small effect on the significance level used in testing, and to find out whether t or v should be preferred. He finds that with equal group sizes, the 5 per cent and 1 per cent levels of significance are very closely approximated by the tests (5.30 per cent and 1.13 per cent) even when the true variances of the two groups are in the ratio 3:1. The bias, as might be expected, is larger when the group sizes are unequal (in his tests 15 and 5, respectively). Powers of tests with equal and unequal group sizes and with associated variances of varying ratios are also tabulated. An important rule-of-thumb is stressed: In the case where an investigator is obliged to take samples of different size, he is well advised to take the larger sample from the population with the larger variance. Finally it turns out that for unequal variances, v is less affected than t in the examples considered. Therefore, if there is any reason to suppose that unequal variances are present and when sample sizes are unequal, the investigator is advised to use v , rather than t (at least until more examples are carried out). This is a most happy outcome because v is easier than t to remember, to print, and to teach, and the regular t -table still works for it.

A related problem is this: An experimenter plans to use a number of experimental groups (one to ten) and a number of control groups (one to four); he knows roughly the magnitude of the standard deviations in the experimental and control sections of the experiment; what proportions of his total group of subjects should he assign to the control and experimental groups to

maximize the opportunity for observed differences to be significant? Kurtz, Jaspen & Ash (53) provide tables to help answer this kind of question. Considering one to four control groups, and one to ten experimental groups, makes 40 combinations of numbers of groups in all. Then for these 40 combinations, they tabulate the optimal sampling proportions for groups with standard deviation ratios of 0.1 to 2.0 by steps of 0.1, and for ratios of 2.5, 3.0, 4.0, 5.0, and 10.0. Of course, in a particular experiment, probably an investigator will not know the ratio of the standard deviations to any accuracy, but he may have observed a factor in standard deviations of say roughly two to one, and these tables offer him a way to capitalize on that knowledge.

Quite often experimenters worry about the effects of nonnormality on the F test, or about effects of unequal variances on the F test. As part of a larger study on the effects of nonnormality, David & Johnson (23) show that when within-group variances are unequal the situation most likely to lead to wrong judgments of significance will be when one of the group variances is very much larger than all the others. For instance, with four groups with variances 1, 1, 1, and 3, instead of .05 the actual significance level is .063, and instead of .01 the actual level is .021. The general implication is that departures from assumptions may have little effect at some points of the distribution function, say near the mean or median, but far out in the tails of a distribution, percentage errors get to be large and the model may just no longer be appropriate. There are no numerical results yet available on the more general nonnormality problem, except that if samples come from the same nonnormal kind of population and are of equal size, the effect on t and F tests will be "slight." (For those able to cope with fairly heavy amounts of mathematics, Bradley (6) provides corrections for nonnormality in the use of two-sample t and F tests at high significance levels.)

Pearson & Hartley (72) have provided power curves for the F test (at .05 and .01 significance levels) for all pairs of degrees of freedom with

$$\nu_1 = 1, 2, 3, 4, 5, 6, 7, 8 \quad \text{and} \quad \nu_2 = 6, 7, 8, 9, 10, 12, 15, 20, 30, 60, \infty$$

where the first set of degrees of freedom is associated with the numerator of the F test, while the second set is associated with the denominator. These curves permit quick estimation of the probability of Type II errors in planning analysis-of-variance design experiments.

Since there are so many possible pairs of degrees of freedom in the F distribution, most tables contain only the most frequently called-for percentage points—.05 and .01. Mood (66) gives five points (.90, .95, .975, .99, and .995) and Merrington & Thompson (62) give many points. Burke (10) provides a method for computing the significance of any F value observed, and provides examples of the computation. Probably the choice between doing the computation and consulting the Merrington & Thompson tables will depend on the availability of the tables.

SUGGESTIONS FOR MODELS OR TRANSFORMATIONS

Cronbach (20) in 1949 proposed methods for testing patterns observed in Rorschach protocols, involving patterns made up of not more than three variables simultaneously. Special statistical difficulties are encountered in analyzing Rorschach data, as Cronbach recognized, because of the influence of total productivity on subscores. Eichler (32) proposes using the square-root transformation on Rorschach scores as a method of reducing this difficulty. When the scores have been normalized using the transformation, he suggests that it is appropriate to use an analysis of covariance, and that this analysis is useful when there is correlation within groups of subjects between productivity and Rorschach score. Block, Levine & McNemar (3) suggest transforming Rorschach scores to standard normal T scores, but they address themselves to the problem of testing for group psychometric patterns. An example might be a search for personality patterns that differed from culture to culture, say. They recommend a three-way analysis of variance (groups, individuals, and Rorschach scores). To test whether observed profiles are different from one another, an F test is proposed:

$$F = \frac{\text{group} \times \text{Rorschach-score interaction}}{\text{sum over the groups of individuals} \times \text{Rorschach-score interaction}}.$$

No example of the procedure is given except an algebraic one.

In cases where a square root transformation seems reasonable (like those described above) and also for people interested in the arc sine transformation for binomial data, Freeman & Tukey (37) have provided some modified square root and arc sine transformations which seem to accomplish better the goal of the original transformations. The analysis of variance ordinarily uses the assumption that the normal populations from which the cell observations are drawn have equal variances. For some kinds of data, the variances depend on the means. Counts such as those arising from binomial or Poisson data (or Rorschach data) usually have this unhappy property. Freeman & Tukey have discovered that the variance-stabilizing properties of the square root transformation are improved by using

$$\sqrt{x} + \sqrt{x+1}, \quad x = \text{observed count}$$

for Poisson data, and the variance of Poisson data so transformed is unity. For the binomial they propose

$$\frac{1}{2} \left[\sin^{-1} \sqrt{\frac{x}{n+1}} + \sin^{-1} \sqrt{\frac{x+1}{n+1}} \right]$$

and data thus transformed have variance $821/(n+.5)$ for angles in degrees. Here x is the count, n is the sample size. Apparently these transformations are better than any previously proposed for stabilizing variances for these distributions. The reviewer's evaluation of such transformations does not

stem so much from the variance-stabilizing property directly, as from the useful known baseline variance provided by the transformed values which can be compared with the residual variance. Knowing the variance, the experimenter can know how nearly he has achieved the natural minimum variation in the situation, and thus how much variation is left to be accounted for by uncontrolled sources of variation. For the practical man this means not only better statistical tests in the sense of making fewer errors, but more important, an experimenter is provided with a measure of improvement in experimental design when he tightens control on different measures. It would be useful to have tables to go with these new transformations that would consist both of the transformed values and the squares of the transformed values. The article has a typographical error in the variance formula.

McNemar (60) comments on the appropriateness of the Latin square as a model or design, and provides an article of the type mentioned in the INTRODUCTION pointing out misconceptions about a statistical method in wide use. Workers have used the Latin square design in instances where order of administration of test was important. McNemar points out that in ignoring order of administration psychologists have overlooked the assumption implicit in the Latin square design—the interactions are assumed to be zero. It would be valuable to have some investigations of the effect of violating the assumptions to differing degrees on this model (like the kinds of tests made to see how differing variances affect the t test mentioned earlier). Larger analysis of variance designs would need to be built with varying amounts of interaction present, and then a parallel Latin square analysis carried through to see how much conclusions were distorted by not using a design capable of showing the interaction.

Information theory is becoming a fruitful source of models for the psychologist. Certain special problems in learning have been handled by Miller and others (63, 64) using some of the concepts from information theory to study learning. Newman (71) has provided a table of $-p \log_2 p$ for p from .001 to .999, so that the calculations necessary using an information model are greatly shortened.

Garner & Hake (38) apply some principles from information theory to scaling problems using absolute judgments. Their article represents both new techniques for scaling problems, as well as application of information theory to psychological problems. They illustrate a procedure for reducing the number of scale categories in accordance with the amount of information being transmitted, in cases where the stimulus is a symbol for a group of events. They also describe a technique for constructing a scale of equal discriminability; this kind of scale results in transmission of maximum information because information received is greatest when probabilities associated with each event are equally uncertain.

Hick (46) suggests the use of information theory to provide a general frame of reference for intelligence and other testing problems, and points out the possibilities of information theory as a model for some tests. Questions

can be reduced, with some labor of course, to incomplete number series. After such reduction, he conceives that the subject receives a signal (the question) and is required to reconstruct the message (question plus answer). One stumbling block is that the information conveyed by the question depends on the subjective probabilities of various answers and these probabilities are characteristic of a given individual (something the subject has never heard of has just about zero probability as an answer). In practice it would be necessary to try out large numbers of questions on many people to get some idea of the empirical probabilities. Perhaps the most interesting result of this way of thinking about the problem of intelligence testing is the derivation of a correction for guessing on multiple-choice questions. This correction is like the one commonly employed: subtract the number of wrong answers from the number of right answers for two alternatives. The chance P of getting a person who knows the answer to a question is optimally $\frac{1}{2}$ —this maximizes the uncertainty. But the chance P can be adjusted by varying the difficulty of a question until the observed relative frequency of success approximates $P + Q/t$, where Q is the chance of getting a person who does not know the answer and t is the number of alternatives in the question.

Anyone interested to a serious and technical degree in the discriminant function as a model will find Kullback's analysis of the problem (52) illuminating. The general problem is to find the "best" linear function for discriminating between two multivariate normals (matrices are used and examples are given). The paper defines the divergence between the two populations in terms of Shannon's and Wiener's measure of information, notes that the divergence is a special case of Mahalanobis' Generalized Distance. Then it is shown that (a) if the variance-covariance matrices of the two populations are identical, Fisher's classical discriminant function is optimum; (b) if the means of the two multivariate populations are identical, the method of principal components is optimum for discriminating; (c) a more complicated special case results in Hotelling's canonical correlation method. Literature is cited.

In the analysis of variance, as in regression, there are many different mathematical frameworks (often called "models"). Sometimes one model is more appropriate for a particular experimental design than another (viz., McNemar's discussion of the Latin square design) even when the data take the same tabular form. The two most commonly employed analysis of variance models are labelled Model I and Model II. To illustrate Model I, consider measuring tapping speed a number of times for the same subject under each of several conditions. Then the score on a particular trial is viewed as formed this way:

$$\text{score} = (\text{constant}) + (\text{gain for particular condition}) + (\text{error}).$$

The constant is a baseline assumed the same for all experiments equivalent to the one being analyzed, the gain or loss occurs because the particular condition helps or hinders tapping, and it is assumed that the "error" is normally distributed with the same variance for each condition. The con-

ditions used (i.e., the factors peculiar to each experimental group being tested) form the whole population of conditions of interest. In Model II an additional assumption is made. The conditions are thought of as a sample from a population of conditions, where the gains and losses are normally distributed with unknown variance. If the experimenter chooses as conditions the addition of 0, 1, or 2 oz. to the weight of the stylus, Model I seems more appropriate, while Model II seems outrageous. But suppose the conditions are different days of the year for a practiced subject; in this case Model II might be more appropriate because the experimenter seems interested in day-to-day variation, which might be a random sample from a larger population of days. The estimation of variances and testing of hypotheses about variances form the class of problems falling under the rubric of "variance component analysis." The journal *Biometrics* has devoted the entire issue of March 1951 (Vol. 7) to this class of problems. The issue includes a survey of the literature by Crump, as well as seven articles contributing new results. This issue should become as famous as the March 1947 (Vol. 3) number of *Biometrics*, an excellent issue devoted entirely to the analysis of variance.

A problem which falls in the class of analysis of variance components is the estimation of the reliability of ratings, and it is greatly complicated by the variety of ways ratings can be used. Ebel (30) suggests and illustrates an analysis of variance solution for this problem.

Considering more general problems about the use of models in psychology, Weitzenhoffer (82) asks: "Why has mathematics (as opposed to mathematical statistics) been applied so successfully to the physical sciences, but not to psychology?" He argues tellingly that the answer is that psychology has too few multiplicative (not ratio) scales. For example, though we might define and measure "subjective work," we have not identified it as the "product" of "subjective force" and "subjective length." Until such relations are available, it is difficult for classical dimensional mathematics to operate, and in the view of Weitzenhoffer (82, p. 400)

the possibility of adequate mathematical representation for any system . . . depends upon the possibility of establishing a system of measurements which is isomorphic with the number system and other mathematical systems.

(An alternative view might be that psychology is not going to have many multiplicative scales outside psychophysics, and that we had better find some mathematics to make the most of what we have.)

In the first of two papers, Bush & Mosteller (12) present a probability model for instrumental conditioning and extinction. The probability that a response will occur during a small time increment is changed by mathematical operators which are introduced to describe the effects of environmental events and the influence of the work required in making the response. The response probabilities are related to experimental measures such as latency and rate, and the model is extended to describe conditioning with various types of partial reinforcement schedules.

In the second paper (13), a model for stimulus generalization based upon earlier work by Estes (33) is presented. Stimulus situations are symbolized by sets of elements and the probability of a response is defined in terms of these sets. An index of similarity of two sets is expressed in terms of their overlap. Discrimination training is described by combining the conditioning and generalization models. No attempt is made to fit experimental data but it is shown that the models lead to curves which resemble those obtained in several experiments. This approach uses probability as a measure, and thereby avoids some of the multiplicative scale problems mentioned by Weitzenhoffer (82), but, of course, the scope of application is thereby simultaneously reduced.

Accident proneness is an issue that any psychologist may be asked to comment on, and one that psychologists are quite likely to continue to study. Mathematical statisticians have tried to contribute, and there is a large body of mathematical models for this problem. Almost any amateur in this field, be he psychologist, statistician, or both, is nearly certain to be error-prone. The excellent critical and survey article by Arbous & Kerrich (2) sums up the story through 1951 for both the nonmathematical and the mathematical reader. With regard to "accident proneness" proper, they decide that (2, p. 343)

our knowledge of this concept has hardly proceeded further, and in some respects has suffered a reverse from the time when Greenwood, Woods, Yule and Newbold undertook their classic studies in 1919 and 1926.

They list and discuss the major psycho-physiological qualities that may affect liability to accidents of individuals working in similar environmental situations: health, age and experience, alcohol, fatigue. Mathematical models for accident proneness are still unsatisfactory because the basic data necessary to verify assumptions have not been gathered (if they can be) (2, p. 364). No one knows whether individuals are differentially liable (accident-prone) to minor accidents; it is difficult to secure uniform reporting (2, p. 368). Correlations between number of occurrences of major accidents in successive periods (yearly?) are of the order of .20 or .30—not much use for prediction—but provide support for a theory of proneness (2, p. 369). Minor accidents do not help predict who will have major accidents (2, p. 370), nor does one type of accident help predict another (2, p. 371). The clinical approach where workers are studied separately and advised how to improve their work looks promising in some businesses, but does not necessarily throw light on accident-proneness as such because inadequate controls have been used. Nevertheless, the unique-person approach (morale boosting campaigns) is about the only remaining lead (2, p. 390).

Although, as stated above, mathematical models for accident proneness are still in an unsatisfactory state, it is sometimes assumed that accidents do distribute themselves according to some Poisson or negative binomial distribution. Burke (9) has suggested two chi square tests for independence

when the Poisson model is assumed. The tests do not seem to be specific to any particular notion of correlation, but test goodness-of-fit when cells and groups of cells have their frequencies estimated from the margins. Their power for detecting correlation useful for prediction may therefore be low.

SURVEYS

In the field of surveys, the issue of whether or when to use one of the many types of probability sampling is just beginning to get interesting. Probability sampling seems to be the only method available for making inferences from the sample to the sample population and thence to the original target population without considerable use of personal judgment. The principal drawbacks seem to be initial cost and interviewing costs compared with quota sampling. Some new information on the question has been reported. A very important factor limiting the results to be obtained from probability sampling is the refusal rate—if the refusal rate is 10 per cent on a Yes-No question, and we find 50 per cent of Yesses in the responding 90 per cent, then the actual per cent of Yesses in the sample is an unknown number between 45 per cent and 55 per cent. If one regards these as two sigma limits for a binomial, the error is already equivalent to that of a binomial sample of 400; and we have not yet added in the sampling error. The Gallup poll has tried probability sampling in 10 countries. Durant (26) summarizes some factors influencing the choice of probability or quota sampling: in probability sampling a failure rate of 15 per cent to 20 per cent should be expected even after three call-backs [this agrees with figures cited by Durbin & Stuart (28)]; interviewing is best done in the evening [this disagrees somewhat with Crossley & Fink (22)]; the cost of interviewing in probability sampling is three to five times the cost of quota sampling. Durant reports that comparison of the Norway quota and probability samples gave no divergences greater than three per cent.

Hauer & Meier (45) think that area-probability sampling is better suited for household-based items than respondent-based items. They list the following drawbacks for probability sampling in public opinion measurement: (a) coverage cannot be met in time schedules, (b) wearing out of the individuals or households, (c) answers are much the same as in quota sampling.

The reviewer notes that there has been little discussion in the literature of the applicability of using the persons not found in a first survey as the not-at-homes to be hunted down ruthlessly by a trained crew for a second survey, then the second survey not-at-homes are given to the trained crew for inclusion in the third survey, etc. Nor is the applicability of the method of subsampling not-at-homes with a trained crew much discussed [but see Cochran (16)]. Further, the point seems not often to be made that the 15 to 20 per cent nonresponse from the probability sample implies that some special one out of every five or six persons or households in the original target populations is not being represented in surveys. Agreement between incomplete probability sample results and quota sample results may imply that

only a slightly different proportion of the target population is being omitted by the quota sample. The reviewer also feels that there needs to be better distinction in reports between direct refusals, not-at-homes, moving away, etc. The 15 or 20 per cent nonresponse for probability sampling may not be very high if we have a strict measure of refusal rate for quota sampling. It will be years before sufficient evidence is in on the probability sampling issue. Meanwhile we are thankful that some organizations are beginning to bring solid factual information to the workers in this field. We need less argument, more and better data.

In all kinds of surveys, interviewer variability is a sizeable part of total statistical error. In an elaborate experiment comparing response rates of experienced and inexperienced interviewers, Durbin & Stuart (28) find that inexperienced interviewers have more nonresponse than experienced ones. The respondents were preselected by the survey. Aside from the training variable, the response rate was relatively constant over variation in other factors such as age or sex of interviewer and subject, form of questionnaire, district. They found that interaction between variables was remarkably absent. The experienced interviewers were persons who had been interviewing for one organization for some time; the inexperienced interviewers were students who had done no previous interviewing and whose only training was the briefing given to the whole interviewing staff before beginning the project at hand. The results also bear on the probability sampling issue. Stock & Hochstein (77) suggest the following methods in addition to training to reduce interviewer variability: more supervision, restricting the freedom of interviewers in selection of respondents, greater care in question design (more factual, less judgmental), choosing interviewers more like the population being studied. They also suggest decreasing the number of interviewers as a method of reducing variability—this factor seems to the reviewer a little unclear, especially since Hansen *et al.* (44) make the optimum number of interviewers an empirical question depending on the study. A symposium entitled "Modern Methods in the Sampling of Human Populations" was held by the American Public Health Association. Papers by Cochran (16), Cornfield (18), and Hansen & Hurwitz (43) make good nonmathematical reading.²

In addition, the following related articles are reviewed by title: Crossley & Fink (22); Feldman, Hyman & Hart (35); Hansen *et al.* (44).

Matthai (58) describes some methods of estimating population parameters which may interest survey workers. Suppose we have data from a bivariate normal population on a sample of individuals of the form: (a) observations on both variables X and Y for some individuals, (b) observations on X but not on Y for some individuals, (c) observations on Y but not on X for some individuals. Then, if X and Y are correlated, it is possible to make esti-

² Reprints of the symposium are available from Theodore D. Woolsey, U. S. Public Health Service, Fourth and Independence Avenues, Washington 25, D. C.

mates of means, standard deviations, and correlations that are better than the obvious ones. For example, the mean of X can be estimated from individuals in groups (a) and (b) but a better estimate of population parameters can be had according to Wilks (84) from fragmentary samples. Now Matthai has extended this result to more than two variables. The method is applied to anthropological data, sample survey data, and to experiments involving partially overlapping groups of individuals.

SCALING

It takes roughly 4 hr. with a scalogram board to scale the responses of 100 subjects on 12 five-category items using the Guttman scaling method. Without a scalogram board, it may take even longer. The boards have some inherent drawbacks in addition to not being widely available: the upper limit on the number of subjects used is 100, and no record is provided of the intermediate steps in the scaling. Apparently it is possible to do Guttman scaling using two IBM machines. Kahn & Bodine (49) provide a method for doing this with complete instructions and wiring diagrams. To use their procedure a standard full-capacity Type 405 Alphabetic Accounting Machine (Tabulator) and a Type 602 Calculating Punch are needed. The method can be used for more than 100 subjects and provides permanent records of the intermediate steps used to arrive at the final scale. They report that for 100 respondents and 12 items with five categories each, it takes about 2 hr. to do the scaling—about half the time necessary using a scalogram board.

A scaling method has been developed and applied by Mosteller & Nogee (70) for measuring utility to individuals of small amounts of additional money income. The theory is essentially that of von Neumann & Morgenstern; the experiment involves the application of standard psychometric methods in a gambling situation. Individual utility curves did not in all cases resemble the standard type of diminishing-returns curves postulated in much of economic theory. Indeed, for a low economic group increasing dollar value corresponded to increasing subjective value (roughly speaking, two dollars was worth more than twice as much as one dollar). On the basis of utility curves constructed for individuals in an initial experiment, it was possible to predict behavior in new and more complicated risky situations. This prediction was better than predictions based on knowledge of odds and money values only.

The following articles relating to scaling methods are reviewed by title: Blum & Dutka (5); McCarthy (59); Mosteller (69).

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THEORETICAL PSYCHOLOGY^{1,2}

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Theoretical psychology is a branch of the philosophy of science. It is not itself either a psychological theory in the sense in which Newtonian mechanics is a physical theory, or, even, the fundamental or the comprehensive psychological theory in the sense in which Newtonian mechanics was once believed to be such a theory in physics. In other words, the philosophy of science, not itself a science, is about science; it is, as one also says, the logic of science or, more precisely, its epistemological analysis. Theoretical psychology, then, is the logic of psychology. It cannot decide, therefore, which of two or several rival psychological theories is the more "fruitful." For such a claim, if it is not either mere advocacy or the rationalization of a hunch, would have to be grounded in what we know, in a scientific way, about the psychology of discovery. Yet, we shall see that a good deal of advocacy and rationalization still go by the name of theoretical psychology.

To survey developments in theoretical psychology is, in the nature of things, a task quite different from that of reviewing an experimental or clinical area. For one, the movement of ideas is slower. Also, many of the papers that appear are either purely ideological or repetitive, not surprisingly, in the absence of the restraints of either clinic or laboratory. More important, a mere "review" that does not sketch in a good deal of background would not make sense to anybody. Such sketches are, of necessity, colored by the views of the reviewer. Thus this review will read more nearly like another of the papers to be reviewed than would be proper in an experimental or clinical area. The bibliography will probably be both shorter and more selective than others in this volume and will range more freely in time.

The philosophy of a science describes analytically the nature and structure of its concepts, of the laws in which they occur, and of the theories into which these laws combine. In the philosophy of psychology virtual agreement has been reached on certain broad principles in these three areas. The basic or, as one also says, undefined ingredients of all concepts (terms) that are characteristically psychological refer to bits of overt behavior, that is, to events that are immediately observable by everybody and not, like so-called mental events or bits of consciousness, only by the owner of the mind in which they occur. Every psychological law properly so called contains at least one such concept; but it may, and as a rule does, also contain members of at least one of two other classes of concepts, namely, concepts which refer either

¹ The survey of the literature to which this review pertains was concluded in April, 1952.

² I am greatly indebted to Professor Kenneth W. Spence for reading the manuscript and making valuable suggestions.

to features of the behaving organisms' physical environment or, in physiological terms, to physiological states or events inside their bodies. The laws of psychology are either statistical or nonstatistical; the latter are, rather misleadingly, sometimes also called causal or deterministic. In either case they are of the same logical nature as the laws of any other science. Either they relate, without explicit reference to time, concurrent traits or they predict the future from the present and, perhaps, the past. In this sense they are all "deterministic" or "mechanistic," not purposive or teleological, though they may, of course, be about mental contents of the kind commonly called purposes. A theory, in psychology as elsewhere, is a set of laws deductively connected; that is, some of these laws, called basic or fundamental or axioms, allow jointly for the deduction of the others, called theorems. And, of course, a theory permits also the deduction of laws as yet untested. No theory both articulate and comprehensive exists at present in psychology. Yet, the construction of such a theory is the ultimate goal of science. Psychology is no exception; nor are there any logical reasons why it could not achieve this goal. From other fields and existing psychological theories, one is, finally, led to expect that the concepts in the basic laws of a worth-while theory are always rather abstract. This means that while they are, of course, defined in terms of the immediately observable, the definitional chains leading to them are rather long and they do not themselves name anything we can see or smell or manipulate like a physical object. Because such concepts occur characteristically in the basic laws of a theory, they are sometimes called theoretical concepts.

The most recent summary of all these principles is due to Spence (37); some of them were in greater detail recently discussed by Brunswik (11), Miller (31), and myself (3). Among psychologists, the most articulate proponents of this general view are the "behavior theorists," Hull, Spence, and their fellow students, who have, in a rather limited area, developed what is at present the most elaborate and articulate psychological theory. Historically, these authors stand in the succession of Watson. Among philosophers, the position is most vigorously embraced by logical positivists. It is, therefore, often spoken of as behaviorism, or neobehaviorism, or logical behaviorism. Yet I believe Spence is right when, in the title of his article, he surrounds 'behaviorism'³ with semantical quotes. For the original Watsonian doctrine is a mixture of logical analyses, of metaphysical biases, and of a dated and rudimentary psychological theory. The position I have outlined retains from this doctrine only what Brunswik calls its objectivism, the insistence that all psychological concepts be defined or, as one also says, operationally defined in terms of manifest behavior. So the old names arouse, perhaps, untoward associations. Be that as it may, since I just spoke of virtual agreement on the

³ In this chapter when a word is mentioned, not used, it is put between single (semantical) quotation marks. Double quotation marks are employed as usual to express emphasis, indicate direct quotation, etc. For example: Watson outlawed such terms as 'purpose' because he thought them irremedially "dualistic."

broad principles of "neobehaviorism," I must next record whatever dissent has recently been voiced.

Concerning concepts, Watson, though he really knew better, did sometimes say that there are no such things as phenomena or mental contents. Common sense knows that this is silly and epistemological analysis (3) bears it out. More important for a science of psychology, many concepts of personality theory either refer to entities in the phenomenal field or contain in their definitions concepts that so refer. I shall for the moment call these concepts phenomenological, since this is also the name of a position which for their sake rejects the objectivist frame of reference. Snygg, who has long held this position, now defends it again, with Combs (35), against Smith (34). Smith makes all the relevant distinctions. He points out that though phenomenological concepts may be legitimate or, even, indispensable, it does not follow that they must be construed as basic or undefined. He admits that we cannot at the moment provide for many of them definitions that go far beyond "Well, the patient said it." But then he reargues effectively the familiar thesis that all such concepts can, in principle, be defined by a more sophisticated use of verbal behavior in conjunction with other variables. Thus the objectivist may, in principle, use phenomenological concepts and will not, if he is reasonable, object to the clinician actually using them without waiting until they are refined to the standards of the laboratory. Brunswik (11), in his discussion of the various ways in which the verbal protocol has been construed, is on this point not as explicit as one could wish. Berlyne (6), on the other hand, is quite clear on the matter and goes on to ask where in the schema of variables that the behavior theorists propose, such phenomena as percepts would find their place. He concludes, correctly I believe, that percepts are always responses, not intervening variables, though they do often temporally intervene between the presentation of a stimulus situation and other possibly more overt responses in which the investigator is primarily interested. But to return to the discussion between Smith and Snygg. Smith distinguishes between the logical thesis that phenomenological concepts are among the characteristic undefined terms of psychology and the psychological thesis, recently also propounded by Snygg & Combs (36), that the phenomenal field of a person at any given moment contains all the determiners of his behavior at this moment. The former does not imply the latter, just as the latter could consistently be held without the former. Besides, the latter is, to say the least, not very plausible in the light of the facts to which the psychoanalysts or, even, members of the Würzburg group have called attention.

Concerning laws, the recent developments in quantum mechanics, which are so much discussed and yet so little understood, demand their toll, in theoretical psychology as elsewhere. London (24, 25) believes that the physicists have abolished "causality" and "determinism," and that psychologists will do well to catch up with them and abandon their futile search for laws so that their discipline can again become what it ought to be, namely, an

art rather than a science. Brunswik (11), in the expository part of his monograph, says once more briefly and incidentally what needs to be said on this score.

Concerning theories, Skinner (33) elaborates his distrust of the behavior theorists' approach to learning into an attack on theory in general. Here is the gist of his argument. Theories require theoretical concepts; but "low order" laws contain only "low order" concepts; and the only purpose of science is the direct discovery of such laws. This purpose is best served by patient search for low order variables that help these discoveries along as, say, better yardsticks and time pieces would have helped Galileo. Theories do not aid the scientist in this search. If, for instance, in my illustration, they cause him to search instead for more abstract concepts that are based on space and time measurements, such as velocity and acceleration, then they are even harmful. He ought to tinker with sticks and clocks. In the area of learning, Skinner claims, the response measure he favors turns the trick; it leads directly to the discovery of all low order laws. This is a factual claim; so I leave it to the experts to judge it on its factual merit. Philosophically, Skinner reminds one of Mach. Fortunately, the belief that an empiricist must not countenance high order concepts and theories has been exploded long ago. In the context of psychological thought, Skinner seems to fear that the behavior theorists' intervening variables commit them either to physiologizing or to some sort of mentalism. The second of these *non sequiturs* is among those that led Watson to his dogmatic peripheralism. The British psychologist Thouless (38) does not fall into this trap. He says, for instance, that the factors of differential psychology are "real," though we have not, as I shall for the moment put it, "located" them either in the phenomenal field or inside the body, and though they are abstract in that they are neither physical objects nor directly observable properties of physical objects. His one reservation in what is otherwise a brief restatement, in the style of the British positivists, of the objectivist view is of a different kind. The present lack of a comprehensive theory that deserves the name is beyond controversy. The several rather crude theories we have are all of limited scope. But there is nothing in the subject matter of psychology that makes the continued coexistence of such isolated fragments either inevitable, or, as Thouless thinks, desirable. Nor are, as he also believes, two different theories of identical scope merely two different ways of saying the same thing. If they were, they would not be two theories but, no matter how different the terms they use may be, logically one. I do not mean to deny that some psychologists suffer from semantic fixations or that some others should be warned against that spurious comprehensiveness which is paid for by vagueness and triviality. But I don't think that this is what Thouless had in mind.

So much for explicit dissent from the principles of the objectivist or, if you please, neobehaviorist position. Within its comfortably spacious rink recent discussion turns on the role actual physiological variables or physiological "models" should play in psychological theories. Simplifying for

the sake of patterning, one might say that a "prophysiological" attack has been developing from two centers. One prong, more or less within the pale of behavior theory, was spearheaded by Meehl & MacCorquodale (30); its philosophical allies are realists (13). The other prong, grown out of a body of experimental work on "Personality and Perception," is led by Krech, whose major statement (21) appears characteristically in a volume with this title (9). It may be called neogestaltist, since it proclaims in the new context the old doctrine of Gestalt. The "defendants," behavior theorists such as Hull, Skinner, and Spence, seconded by logical positivists (3), admit cheerfully that they do not engage in reductionist research and do not use physiological models in their theorizing since they are sceptical about their fruitfulness at the present stage of the game. Yet they deny the charge of being "antiphysiological" because, according to them, no systematic issue being involved, it does not make sense within theoretical psychology to be either pro- or antiphysiological. What they really oppose, they maintain, is the logical blur which inevitably surrounds the hypostatization of a research preference or a hunch about "fruitfulness" into a systematic position. This being the pattern, I turn first to an apparent exception. Brunswik (11), though firmly grounded in the positivist tradition, says in the one recent publication of monograph length many things that cannot but impress the casual reader as "antiphysiological." The impression is deceptive. Yet, metaphorically speaking, the advocacy is in the text, the saving qualifications are in the footnotes.

The objectively correct perceptual response R , or one correct within given limits, to a distal stimulus S , presented either to N subjects or N times under different conditions to one subject occurs only $p.N$ times, p being a certain percentage. The prediction of R as dependent on S (and on nothing else) is, therefore, statistical; or, as Brunswik puts it, a law "focussing" on these two variables is "probabilistic." Nor is this surprising, since S is merely a "partial cause," R merely a "partial effect" within the ongoing process, and since, even if all other partial causes are equal, R is "mediated" by a number of proximal cues from which the organism selects by means of a "cue-family-hierarchy" and none of which is by itself objectively sufficient to "infer" the character of the distal object. Corresponding or, at least, analogous distinctions can be made for all behavior. The logic of the situation remains the same. Only "mediational laws" stated in terms of relatively "molecular" variables can yield nonstatistical prediction of such "molar" responses as perceptual or, say, social "achievement." But mediational laws concern themselves, as Brunswik prettily says, merely with the organism's tactics, not, like those other "wide-arched dependencies," with its grand strategy for survival. Strategy, however, is the thing. Only those probabilistic laws which, though not themselves in any logical sense teleological, reflect somehow more directly the purposiveness of behavior are, therefore, the proper subject matter of psychology. With preferences and enthusiasms as such I have no quarrel. Brunswik's unfortunately have consequences

with which I cannot help quarrelling. He says, for instance, that Hull's molecular bias causes his theory to be "entirely contained within the limits of the organism." If this means anything then it means either that the variables of the geographical environment do not at all enter into behavior theory or that, entering it, they are treated in a certain manner, for instance, that a piece of food is not introduced by its physical characteristics but as a stimulus pattern on the retina or on other receptors. Both assertions are patently false. Brunswik calls his doctrine functionalism. One may doubt the wisdom of resurrecting at this late date the name of one of the "schools." Historically, the word can be justified. For the early functionalists were the first to recognize that since the organism is not a closed system, laws containing both stimulus and response variables, and among these again laws of the process type, are of crucial importance. They were also, together with the Würzburg group, the first to insist on the legitimacy of the kind of variables Brunswik champions. But it is likewise a matter of record that, Brunswik to the contrary notwithstanding, classical or Watsonian behaviorism was not in this respect a regression. For one, Watson was in his time no more an enthusiast of physiological reduction than Brunswik is now. For another, to call his approach molecular in a sense in which molecularity is held to narrow a psychologist's view of his subject, seems strange if one recalls that Watson from the very beginning proposed in molar terms a molar theory of personality. That this theory was rather sketchy and that its basic laws were rather over-confidently patterned after a few laws of classical conditioning is an entirely different matter. Besides, does not even the subject of a conditioning experiment adjust to his environment? Really, there is nothing left to fall back on except that old standby, the artificiality of the laboratory situation. Brunswik even resorts to this argument. Misjudging Watson, he also misjudges the systematic contribution of the molar or purposive behaviorism proclaimed in the early thirties. This second wave undoubtedly helped to combat the silly cliché that behaviorism is nothing but a psychology of muscle twitches. But its only contribution to theoretical psychology was the insight that such terms as 'purpose,' which Watson had outlawed because he thought them irremediably "dualistic," can be defined objectively. Molarity, however, needed then no more to be defended against Watson than it does against Hull today.

The same historical bias is noticeable in a paper Littman & Rosen (23) devote to a semantical analysis of 'molar-molecular.' Distinguishing seven meanings with which this dichotomy appears in the literature, they argue convincingly that even aside from the obvious dangers of ambiguity it has outlived its usefulness because virtually all these meanings are either purely ideological, or very vague, or merely add another name where one less compromised is available. The authors seem to feel, correctly I believe, that the most significant meaning occurs in connection with one of 'reduction,' when one asserts, for instance, that psychology can be reduced to physiology and that in this relation the former is molar, the latter molecular. Generally, one

theory is said to be reduced to another if, after the basic concepts of the former have been "related" to the latter, the basic laws of the former become theorems of the latter. If, furthermore, the units of description of the former are in some specifiable sense, such as spatial extent, larger than those of the latter, then the latter is in this sense molecular with respect to the former, the former molar with respect to the latter. Since this relation is thus determined by the relative size of the units, Littman & Rosen need not puzzle what becomes of it when, say, a physiological state of the whole organism is related to a behavioral feature. The cautious term 'relate' is used advisedly. For a behavioral concept never *is*, in the strict sense of identity, a physiological one. To speak occasionally of "identifying" the two probably does no harm in science; in theoretical psychology it may and, I believe, does lead to disaster. All we can possibly mean is that we know an empirical law stating what goes on inside the organism when it behaves in a certain way. So there always were and there remain two things: what goes on inside, described in physiological terms; and what goes on outside, described by means of behavioral concepts.

Intervening variables are abstract concepts of behavior theory for which no physiological interpretation is offered, either explicitly or implicitly, in spite of Hull's physiologically sounding nomenclature which is merely misleading. Hypothetical constructs are, as it were, intervening variables that are physiologically interpreted or located inside the organism. I speak at this point vaguely of interpreting or locating because to state precisely what is involved is part of the issue. Meehl & MacCorquodale, who first introduced (30) and more recently reasserted (29) the distinction, are of the opinion that hypothetical constructs are preferable to "mere" intervening variables. Rather ironically, Tolman (39), the original proponent of the idea of intervening variables, now agrees with them. So do some others, among them Krech (20) and Skinner (33), each for reasons of his own, on one or the other of the subissues of the controversy. There are all together four such subissues. There is, first, the logical as well as the scientific status of physiological reduction. Second, intervening variables, unless physiologically "located," perch, according to their detractors, precariously somewhere in the middle of nowhere, neither fish nor flesh, neither mind nor body. This, we shall see, is a purely philosophical puzzlement. Third, hypothetical constructs are claimed to be more fruitful than intervening variables. Fruitfulness as such is a matter either of goods delivered or of hunches. So far the goods have not been delivered and theoretical psychology, I insisted before, does not deal in hunches. In this case, however, the argument has a systematic core that belongs, as it were, to the logic of hunches. If, as their proponents claim, hypothetical constructs and only hypothetical constructs carry what they call "excess meaning," and if excess meaning turns out to be a structural or logical notion, then the distinction between the two kinds of variables is significant. That the alleged cash value of such excess meaning is fruitfulness, is, from a systematic viewpoint, rather incidental. Fourth, intervening vari-

ables are discounted as trivial and arbitrary, in the sense in which mere abbreviations are arbitrary. The first two subissues are more closely connected with each other than with the rest. Again, the third and the fourth are two sides of one coin. I shall take them up as I listed them, intermingling report, background, and criticism; and I shall feel the freer to do so since by proceeding in this manner I partly restate, partly expand what is still the only direct reply (3) to the advocates of hypothetical constructs.

Logically and in principle, physiological reduction is a certainty. Every bit of behavior and everything that can, like conscious contents, be defined in terms of behavior has its physiological correlate. More precisely, it has one among its possible correlates, depending on the total state of the organism and those "mediational" alternatives of which Brunswik makes so much. Philosophies which deny or even doubt that are simply silly. Refuting them by bringing out the absurdities they imply is an exercise best left to the epistemological seminar. On this truism logical positivists agree fully with those realists who now plunge, on philosophical grounds, for hypothetical constructs. I know of no nonphysiologizing behavior theorist or logical positivist who rejects or ever rejected the search for the physiological correlates of behavior as "transcendental or 'meaningless,'" relegating it "to the metaphysical limbo on general methodological grounds" (29). Never was so flimsy a straw man tilted against so valiantly on so little provocation. The adjective in the expression 'hypothetical construct' is likewise misleading. That all behavior has physiological correlates is certain; and what we are certain of, or as certain as we are of anything in science, is not in any scientific sense of the term a hypothesis. What we are uncertain of is what the correlates are; and this is putting it conservatively, at least with respect to "molar" behavior. For, scientifically physiological reduction is as yet mostly a program. All one can do is speculate, in a nonmetaphysical sense of 'speculation,' what the correlates might be. The result of such speculation is a (physiological) model. This, by the way, is the only good use of 'model' in theoretical psychology of which I can think. To call, as Miller (31) does, every rudimentary theory a model merely invites confusion by adding one more ambiguous word where there are too many already. There are also important logical differences between physiological models and what, in the philosophy of physics, is sometimes called the particle or corpuscular model. To drag the latter into the discussion merely increases the logical blur. A hypothetical construct, then, is simply an intervening variable identified, in a sense of 'identification' I have explained and criticized, with an entity of a physiological model. Such models range widely, from being continuous with what is actually known in physiology to trivializing vagueness and generality (19). Those at the concrete end of this range are, within physiological psychology, obviously helpful or, if you please, fruitful hypotheses in the clear-cut sense in which the statement of a not yet established law is called a hypothesis. Their historical and logical prototype are the theories of vision and hearing which aroused so much interest among psychologists two and three

generations ago. The most notable recent effort, very broadly conceived but not, as far as I can judge, unduly speculative, is Hebb's (16). So it is perhaps worth mentioning that he concedes roundly the logical soundness of a non-physiologizing behavior theory.

According to their protagonists hypothetical constructs are or involve existential hypotheses. Some philosophers (13) use this phrase, *existential hypothesis*, in ways I do not understand. Within the philosophy of science it has one and only one clear meaning. An existential hypothesis is an assertion that something of a kind which we know exists, made either before we know that it is true, as in the case of the jack in a box that has not yet been opened, or if we can confirm its truth only indirectly, as in the case of, say, a pimple on Caesar's nose, which we could not inspect but would have to infer from old manuscripts and statues. The lower case *s* and *r* that occur in behavior theory represent, without being hypothetical constructs, existential hypotheses in this sense.

Only individual physical objects, stones, trees, animals, nerve fibers, occupy literally a place in space. Their properties and the relations that obtain among them do not. This is as true of characters which we immediately observe as of the more "abstract" ones. Logically, there is no difference in this respect between colors and, say, boiling points or friction coefficients. A colored object is in space; looking at it, one "sees" its color directly and thus comes to think and speak, loosely, of colors being in space. The mistaken belief that only what is thus "placed" or "concrete" is "objective" or "real" has been the source of much bad philosophy. That relations, though their instances are not thus localized or, as Titchener put it, palpable, are yet "objectively" there, physically as well as in the phenomenal field, is indeed the one incontrovertible corner stone of the Gestaltist edifice (2). Most abstract or theoretical concepts, by the way, are either explicitly relational, like 'friction coefficient,' or they name, like 'boiling point,' relational properties, that is, characters which, though they are attributed to a single object, involve, as may be seen from their definitions, relations to others. I can see no difference in all these respects between the boiling point of a certain piece of wax and the strength of a certain habit, as the behavior theorists define it, in a certain rat. The central issue here is the general status of "abstract" terms or, rather, that of their referents. This is the reason I called the puzzlement about the "locus" of intervening variables philosophical. If some doubts still linger, they will perhaps yield to a few further remarks about the notion of a property in general and that of a state in particular. Being partly white, partly black, partly of metal, partly of plastic are what one ordinarily calls four properties of my pen. Its having been bought at a certain store or having had a certain temperature yesterday are not ordinarily thought of as two properties of it. Logically, there is again no difference among these six properties as such; they are all equally properties of my pen. Which among the indefinitely large numbers of properties of an object we single out and speak and think of as its "properties" depends on our interest. In science a concept

is interesting if and only if it occurs in laws. A dispositional property is that of behaving in certain ways under certain circumstances. A state, finally, is again nothing but a property of an object or a system of objects, dispositional or otherwise, though, as the term is customarily used, more often than not one that changes in time. Take, as an illustration, the simple proposition that tempered steel is flexible. To be tempered, that is, having been dipped into cold water when hot, is not ordinarily thought of as a property. Yet it is one and, in the case of steel, even an interesting one, because of the law our proposition states. (And this is why it has a name.) To be flexible is a dispositional property. Both properties are of the sort also spoken of as states. What the law asserts is thus the concurrence, in steel, of these two states. To "identify" them, thinking of them because of this law as one, is strictly speaking nonsense, though one could, of course, rather clumsily and unnecessarily, define a further state, that of being both tempered and flexible. Another state is more interesting. If we know enough physics, then we also know the molecular structure of tempered steel. The point is that, logically, this is a third state, and the concurrence of all three a further law. What goes for a piece of steel goes equally for a rat. At least, I see no logical difference between the former having been tempered and the latter having been starved for 24 hr.; between the former being flexible and the latter approaching and consuming food if presented with it; or finally, within the context of the illustration, between the molecular structure of the former and the physiological correlate of a day's starvation in the latter.

Concerning the third subissue, excess meaning, I shall not argue that hypothetical constructs don't have it. They do, provided the model involved is sufficiently concrete or, at least, articulate and not entirely *ad hoc*. Rather, I shall argue that intervening variables, too, have it; more precisely, they acquire it automatically when they are put to use. One of the clearest instances of "excess meaning" is furnished by the classical kinetic theory. So I shall, again disregarding the logical differences between a physiological model and the corpuscular model of a gas, use the latter to explain first of all what the term means. The kinetic theory attempts a reduction (which, as has since transpired, has its limits) by relating the gas and its properties such as volume, pressure, and temperature on the one hand and, on the other, a swarm of particles and their mechanical properties (more precisely, the sums or averages of these properties). At any given state of the theory such relations are specified for some of the two kinds of variables; and some laws obtaining among the gas variables are derived from the laws of mechanics which, by the rules of the game, hold for their correlates. But there is also, at any given state of the theory, the possibility of specifying further relations and of deriving further laws about the gas from the laws of mechanics which the theorist finds, as it were, ready made. This possibility is what is meant by the excess meaning of the model. Clearly, hypothetical constructs may carry excess meaning in this sense. Yet, it is worth noticing again, with Leeper (22) in his enthusiastic review of Hebb (16), that "we just don't know

enough about neurological phenomena to let that knowledge cast any vote as to what psychological principles are superior to what other psychological principles." Another much humbler instance throws some further light on what the notion involves as well as on the present state of psychology. Whenever clinical terms such as 'anxiety' or 'rigidity' are introduced as names for states or behavior patterns objectively defined and produced under laboratory conditions, there is, implicitly or explicitly, the expectation that some of the laws that are believed to connect the clinical terms will be found to obtain among their newly baptized namesakes. Research is then guided by such clinical excess meaning. This sort of thing shades over into whatever benefit clinical psychology itself receives from literary characterology, imaginative insight, or what have you. There is nothing wrong with all this; when it comes to heuristic guidance the theory of personality can as yet hardly afford to be choosy. Only, as long as the mechanism is not recognized for what it is, it is within the science of psychology a hindrance rather than a help, since it leads to the psychological variant of anthropomorphic thinking. The excess meaning of the intervening variables of behavior theory flows from a different source. To appreciate it requires some preparation.

The fundamental laws, principles, or axioms of a theory are always several, never one. Some of them describe so-called elementary situations, in which only a very limited number of variables interact. At least one is a composition rule, stating how to obtain the laws of complex situations in which many variables interact from those of the elementary situations into which any complex situation can be conceptually decomposed. Even a theory logically as simple as Newtonian point mechanics (astronomy) can be analyzed in this fashion. The one elementary law in this case is the attraction formula between two mass points; the one composition law is the so-called vector addition of forces. An elementary law contains as a rule parameters that make it applicable to a number of situations which are phenomenally quite different from each other such as, in the Newtonian instance, planets, comets, and double stars. The composition law always allows for the derivation of a large number of laws for all kinds of complex situations. A theory thus always carries excess meaning in that it predicts laws quite different from those on which it is based. No "model" is required to secure this one characteristic feature of a "fruitful" theory. Conversely, a theory is in difficulty if even one of the laws it predicts is not borne out by observation and experimentation. In this sense all theory or, if you please, all science is merely an "abbreviation" and "circular;" to say that it is, is not saying anything in particular and is otherwise rather confusing. The intervening variables of behavior theory are used in the formulation of its fundamental laws. This, in fact, is their one important use. Again, the theory has both kinds of laws, those covering "elementary" situations and composition rules. The first kind is based on certain of the simpler phenomena of classical and instrumental conditioning; the second kind contains such principles as that which Hull, with an unfortunately misleading name, calls afferent neural in-

teraction, the rule governing the "summation" of habit components, and the law stating what happens if incompatible response tendencies are present. The complex situations for which the theory has attempted predictions are, for instance, discrimination and maze learning. It follows that as soon as the intervening variables of behavior theory are put to use they acquire automatically excess meaning. Why, then, are they now dismissed as "mere abbreviations?"

Tolman (40), when he first introduced the notion of intervening variables emphasized two things. For one, he pointed out that they provide natural definitia for such terms as 'drive' or 'purpose,' which Watson had rejected as unduly teleological or, even, mentalistic. For another, he stressed the abbreviatory convenience which they undoubtedly provide in talking about any particular relation in behavior theory. But nothing he then said, as it seems irremediably fixing the ideas of some, precludes the quaint notion that the goal of behavior theory is the discovery of one single formula, $y=f(x_1, x_2, \dots, x_n)$ with y standing for the response, the x for the conditions, to cover all possible behavior in all possible situations. Psychology does not, any more than does physics, search for such a single world formula. Rather, it looks, like all science, for a limited number of formulae, its basic laws, from which an indefinite number

$$\begin{aligned} y_1 &= f_1(x_1, x_2, \dots, x_n) \\ y_2 &= f_2(x_1, x_2, \dots, x_n) \\ y_3 &= f_3(x_1, x_2, \dots, x_n) \\ &\dots \dots \dots \end{aligned} \quad 1.$$

can be derived, each y standing for a different response. Different items of behavior in one and the same situation as well as different aspects of one such item are different responses in this sense. So is the same kind of item when it occurs in two situations which are different in that they require the application of different formulae of 1. If the independent variables are counted through, as is done here, from 1 to n , then their number is obviously very large, and hardly any of the formulae in 1 will actually depend on all of them. This leads me to a final, purely mathematical comment on this business of mere abbreviations. Consider the system of equations

$$\begin{aligned} z_1 &= g_1(x_1, x_2, \dots, x_n) \\ z_2 &= g_2(x_1, x_2, \dots, x_n) \\ &\dots \dots \dots \\ z_m &= g_m(x_1, x_2, \dots, x_n) \end{aligned} \quad 2.$$

in which, in the psychological application, the z represents intervening variables. If $m < n$ or, in the case $m \geq n$, the rank of a certain matrix is $< n$, then it is a very remarkable and intricate mathematical property of 1 that the y can also be written as functions of the z . So much for the fourth subissue, the charge that intervening variables are merely arbitrary abbreviations.

I conclude, then, that aside from the potential fruitfulness of physiologi-

cal models, which is not a systematic question, the whole controversy of intervening variables versus hypothetical constructs is a pseudoissue, the distinction itself a pseudodistinction.

Not the least danger of a pseudodistinction is that because of the logical blur its terms tend to become charged with all sorts of extraneous meanings, thus further increasing the bedevilment of the unwary reader. A paper by Marx (28) is a case in point. Here is the gist of his argument. He recognizes that psychologists must try to use concepts that are objectively defined, whether they are, as I use the terms, intervening variables or physiological variables. Terms so defined he calls "intervening variables." Yet we cannot afford to neglect the heuristic value of terms which are without such definition borrowed from every day language or clinical practice. These terms he calls "hypothetical constructs." It follows that while we had better admit "hypothetical constructs," the goal must be to refine them into "intervening variables." One way of doing this is to base the definition of new objective variables on the differences between experimental (E) and control (C) situations. An experimental illustration is offered. Both E and C animals are equally shocked and given access to food. Only the C animals can terminate the shock themselves by jumping off the grid; they consume more food. The verbal tag "hypothetically" suggesting itself being 'sense of helplessness,' a new variable, objectively defined in terms of restraining conditions, is tentatively introduced and given this name. Marx believes that it represents a new "unorthodox" type of intervening variable, which he calls E/C. It is nothing of the sort but, rather, a newly discovered directly manipulative variable; if it were a little more abstract it would be a very orthodox intervening variable, if the term is used as the participants in the current controversy use it. Nor does Marx's meaning of 'hypothetical construct' agree with theirs, though there is, in this case, a certain overlap. Yet the paper is an attempt to mediate that controversy. What Marx actually discusses once more, very interestingly and perceptively, is the logic of the discovery of new manipulative variables.

In the writings of the neogestaltists the advocacy of physiological models is, on the face of it, as vigorous and as prominent as in those of Meehl & MacCorquodale. When it comes to intellectual motives, there is, I believe, a subtle difference. What is here primarily attacked in the fundamental logic of behavior theory; what is defended is the Gestaltist philosophy of science in general and of psychology in particular. The physiological model is valued mainly because it is thought to provide evidence, or at least support, for the alleged superiority of this philosophy. Naturally, everybody welcomes allies and tries to oblige them. So the intervening variable is also attacked, incidentally, as it were. There is nothing forced or strained in this strategy; it merely continues the tradition of that physiological speculation which is so characteristic of the classical Gestaltists. In discussing these matters I find it again necessary to proceed rather discursively, intermingling background material, report, and criticism. Since the main statement of the neogestaltist

position is contained in three papers by Krech (19, 20, 21), I shall organize my comments around them, particularly the earliest (21). The positivistic criticism of Gestalt being rather familiar, I shall without further reference draw upon what I have published before (4, 5) as well as on a recent statement of my views by Madden (27). Another part of the context of Krech's and other papers (8, 10, 12, 26) I shall discuss is a large body of recent experimental work on "perception." I need not and I could not cite all this literature; I refer instead once more to a symposium (9) that examines its theoretical significance. But I must say a few words about the general character of this work on "perception" and, first of all, about the systematic status of such terms as 'perception,' 'learning' and 'motivation.' In this I partly follow Graham (14).

The essential terms of a science refer to the various features of its subject matter. 'Mass,' 'velocity,' 'temperature,' 'heat' are instances of essential terms of physics. In addition to these the vocabulary of a science usually contains another group of words which I propose to call its chapter heading words. 'Mechanics,' 'thermodynamics,' 'optics,' 'electromagnetism' are among the chapter heading words of physics. These terms are unessential and dispensable. Whatever meaning they have is largely denotational and either conventional or due to historical circumstance. They serve abbreviatory purposes of convenience; but if they were omitted nothing would be missing from the body of science. The only logical point that needs to be made about them is therefore negative, namely, that nothing can possibly come from building one's logical analysis around these terms. To grasp this clearly, let us first look at physics. So far as I know it has never been argued either that the term 'optics' became logically objectionable or that it lost its convenience when it was discovered that optical phenomena are, theoretically, merely a special kind of electromagnetic events. Similarly, after it was noticed that the sum of the "mechanical" and "thermic" energies of certain closed systems remains constant (first principle of thermodynamics), physicists continued to use the chapter headings 'mechanics' and 'thermodynamics' exactly as they had used them before and without arguing whether the first principle, in which of course both "mechanical" and "thermodynamical" concepts occur, falls properly either under the one or under the other. Also, it remained as true as it was before that the laws for large classes of systems contain only "mechanical" variables, though these laws appear now theoretically as special cases and though we understand now better why they are what they were always known to be, namely, like virtually all laws, idealizations (frictionless motion, etc.). If a special name is wanted for this situation, one may speak of the relative autonomy of mechanics. Such relative autonomies are among the reasons for the continued usefulness of some chapter heading words. If one speaks, as I just did and as everybody does, of mechanical and thermodynamic variables, then one uses the chapter heading adjectives of a science for a classification of its variables. What goes for the nouns goes for the adjectives as well. In itself such a classification is of

no systematic significance whatsoever; it is not logically the first step in the construction of a theory; and it does not either explicitly or implicitly determine or prejudge the structure of this theory. Quite to the contrary. Whatever meaning that is not merely conventional or historical such a classification has is derived from the structure of the theory in which these variables occur, for instance, from the relative autonomy of a group of laws which contains no other variables than those of a certain class. In particular, as we have learned from the first principle of thermodynamics, it does not follow that two concepts differently classified may not occur in one law. Generally, whatever relative autonomies there are, as well as what they are, is entirely a matter of fact. No *a priori* assertion, either positive or negative, is possible.

'Learning,' 'motivation,' and 'perception' are among the chapter heading words of psychology. There is a difference, though, between the first two and the third. 'Motivation,' for instance, or, perhaps better, 'the laws of motivation' can only refer to a group of formulae obtained by keeping in the general laws of behavior theory all manipulative variables constant, except those that are traditionally called motivational. Similarly for 'learning.' The use of 'perception' varies widely. Some would like to say, for instance, that the behavior of one who, having escaped persecution, comes face to face with the symbol of his erstwhile persecutors, say, the swastika, is different from those of others because he "perceives" this object differently. Used thus broadly, 'perception' means everything and, therefore, no longer anything in particular. 'Perception psychology' becomes, by definition, virtually synonymous with 'psychology;' the term has, as it were, conquered itself to death. In a more specific and, I think, quite reasonable usage, 'perception' is roughly comparable to 'mechanics.' One may call perceptual those verbal responses and receptor-postural adjustments, or mental contents defined or definable in terms of such, by which a subject describes or discriminates the physical features, however "elementary" or "complex," of his physical environment. Within rather wide limits most of these responses depend most of the time only on the physical environment, thus giving rise to one kind of those laws in which Brunswik is so exclusively interested. The phrase "within rather wide limits" indicates that in this case we need not, as with learning and motivation, keep all other variables constant; we may simply disregard them. In other words, there is relative autonomy. The new experimental work in perception explores and establishes the limits of this autonomy. Scientifically it is, to my mind, most interesting and intriguing. Logically only two points need to be made about it. For one, it does not at all invalidate the older results on "perception." It merely means that these results, for instance, the many findings of the classical Gestaltists, will now theoretically appear as "special cases," valid within those rather wide limits I just spoke of. For another, the new results do not in principle amaze the behavior theorist; nor do they cause him any scientific difficulties within his theory. For, in behavior theory the perceptual response is just a response among responses; and the dependence, in principle, of all responses upon

"motivation" and "learning" is rather notoriously a basic scientific (not logical!) tenet of behavior theory. Bruner & Postman (10), we shall presently see, recognize the logic of this situation with admirable clarity and detachment.

The main claims of Gestalt are three. First, there is the thesis that some relations are introspectively as well as epistemologically irreducible. Second, an "isomorphism" is held to exist between an individual's brain field, his phenomenal field, and (a part of) his physical environment. Third, there is the doctrine of wholes with its corollary that since there are wholes, science cannot be "additive," "elementaristic," and "mechanistic." Here is the strategy of the positivistic criticism. Some points are immediately conceded as entirely noncontroversial. The introspective irreducibility of some relations, for instance, has not ever been denied by anybody except by Wundt and some of his students and predecessors. It is difficult indeed in following the development of Gestalt not to feel sometimes like an unwilling witness of the old anti-Wundtian rebellion, ritualistically or compulsively reenacted again and again. Some other points consist of an uncontroversial core surrounded by a large mass of vague and elusive accretions. For instance, nobody doubts that the physiological correlates of two different states of consciousness must be different. Generally, the physiological structure must possess multiplicities and variabilities commensurate with those of the phenomenal field. This is the small nourishing core of the isomorphism doctrine. The rest is speculation, physiological or otherwise and for the most part desperately vague, about some ill-defined kinds of similarity among the three fields. The third claim is perhaps the most central. Again, the positivist strategy is not to deny those "holistic" features of the world or its scientific description which so impressed the Gestaltists that they came to believe they discovered them and are the only ones who appreciate them. Rather, the positivists state these very same features within the framework of their own logic; then they show that this statement does full justice to what is the case and to everything that makes sense in the Gestaltists' doctrine. For, their doctrine being a philosophy rather than a scientific theory and a philosophy being essentially a way of words, one best refutes them by first saying in one's own language what they believe can be said only in theirs and by then analyzing the confusions that led to this mistaken belief.

The formula that wholes are more than the sums of their parts has four important good meanings. The first is that what I called the composition rules of a theory are themselves empirical laws, so that, for instance, the law of the process undergone by two interacting systems cannot be deductively derived from the two laws alone, which describe the processes the two part systems would undergo in isolation. The corresponding bad meaning is the belief, propounded with varying degrees of explicitness and confusion, that there are no composition rules; that every new complexity, or at least some levels of complexity, introduce some ill-defined novelty or emergence.

The second good meaning is that every closed system is an instance of dynamic interdependence or interaction. This means that as long as we do not know the formula for the process of a closed system, we must be prepared to find that the value at a certain moment of *any* of its relevant variables actually depends on the values of *all* these variables at an earlier moment. The corresponding bad meaning is the dogma of total dynamic interdependence, that is, the *a priori* denial of the existence of relative autonomies and of all similar or related features of theories and of the closed systems to which they apply. The third good meaning is that the adequate description of the state of a system contains, in addition to the adequate descriptions of its subsystems, also the relational statements necessary to determine how the latter "lie to each other." The worst thing here is the illusion that only Gestaltists appreciate the point. Newton, as far as I know, did not ignore the distances between the sun and the planets. The fourth good meaning is best explained by an illustration. Other things being equal, a subject's response R to a stimulus S_1 presented by itself may be and sometimes is different from the response R' , made to S_1 , when the latter is presented as part of a pattern, say, simultaneously with S_2 , as in color contrast. The point is that S_1 itself is the second time exactly what it was the first and that one can nevertheless explain why the second response to it is different from the first. The corresponding bad meaning is, accordingly, the belief that S_1 itself has changed merely by becoming the part of a whole, this being one of those fabled wholes that determine the nature of their parts. I shall show that Krech's argument suffers from several of these bad meanings.

Steeped as they are in the philosophical culture of the German universities, the classical Gestaltists have always insisted that, on epistemological grounds alone, the "basic data" of psychology are phenomenological. Köhler's (18) latest statement is as explicit as any on this point. Krech, of another generation and another culture, is sufficiently close to the objectivist frame to waive the issue, granting the approximate equivalence of what he calls the two alternative sets of basic data, those of behavior and those of experience. Presently it will appear, though, that, true to the tradition in which he stands, he actually tends to think in terms of the phenomenal "basis." A historical aside of his on these matters seems to me very questionable. He considers the advocates of the behavioral and the phenomenal bases as the contemporary representatives of the psychologies of act and of content respectively. Brunswik (11), for reasons of his own, claims the same continuity. Both authors could cite the authority of Angell (1), who, looking for allies and a pedigree when functionalism was young, claimed the act psychologies among its ancestors. The historical facts, so easily forgotten or misunderstood at a time when the objectivist frame is dominant, do not bear out this pattern. Whatever connection there is—and there is undoubtedly some—is rather superficial; it belongs, as it were, to external history; and it has very little to do with the ideas themselves. For, as the word 'content' is now being

used, acts are merely a special kind of mental contents (2). Thus, it is more confusing than enlightening to relate, for the sake of historical patterning, the classical act to the activities of the biological organism.

Surveying the present status of psychology Krech finds it deplorably fragmented. There is, as he sees it, not one theory; there are three, dealing with motivation, learning, and perception respectively. Each has its own set of variables; each its own isolated set of laws. The three classes of variables are mutually exclusive. Thus, in order to decide which laws or theory apply in a given situation, he thinks one would first have to determine to which class the variables that occur in it belong. This, I submit, is as neat an instance as any of a rationale leading to what I have shown to be the mistaken overestimation of the logical significance of chapter headings. Besides, as I have shown, Krech's three chapter headings are not really coordinate. The first two, learning and motivation, do not indicate an area of relative autonomy as does the third, perception. Be that as it may, Krech, quite consistently within his rationale, attacks the alleged tripartition of theory by arguing that the corresponding classification of the variables is untenable. The two arguments he offers reveal the proclivity to phenomenological thinking to which attention was called before. First he remarks, quite correctly, that a subject would include perceptual, cognitive, and motivational material in his introspective account of an "emotional" situation. If I may make the argument more explicit than it actually is, it continues as follows. Since variables from all classes are present in this and, presumably, in any situation, no law from any of the fragmented theories can apply to it; for all variables present in a situation are actually dynamically relevant in any process or part of a process that develops from this situation. This is, clearly, the thesis of total dynamic interaction. Krech's second argument insists that in some cases it is not even possible to pigeonhole the data as such. As an illustration he takes Lewin's valence. "The *perception itself*, under certain conditions, has invitation character, and this invitation character is as much a genuine perceptual attribute as is color or shape. It is not a matter of gluing on a 'plus' sign to a 'perception' " (21). More simply, when I am hungry, food "looks" good to me. The reader will remember what was said about the swastika. Aside from that, I don't doubt that in this case an introspective protocol in the Würzburg or stream-of-consciousness style may contain some such sentence as 'This looks inviting.' But I do not see any reason either why under the analytic set of Wundt-Titchenerian introspection such a content should not be decomposable. In saying this, however, it is not my intention to extol, under the sway of my "elementaristic" sympathies, the one style of introspection at the expense of the other. The point is, rather, that I wish to deprecate the systematic significance of either within the frame of an objectivist psychology. For a further clarification, I suggest that the morsel which looks so inviting is one of those wholes that determine irreducibly the nature of their parts. The only difference between this case and that of color contrast, which I chose as an illustration before, is that here the rest

of the context is not an external stimulus but a hunger. As for the reference to Lewin, whom Krech acknowledges as his master, it is, I fear, rather ironical. As everyone knows, Lewin, who was very fond of diagrams, always drew two; the first representing what he called life space and others call psychological environment; the second, loosely located somewhere within the first, the multicellular tension system of the person. Thus it became for him a "theoretical" question whether a certain state of affairs was to be represented by, say, a barrier in the life space or, perhaps, by the impermeability of a membrane in the hydraulic analogy of the person. This surely is a hang-over from that old dichotomy, which Krech is so anxious to avoid, between the senses and the intellect on the one hand and the will and the emotions on the other. (These are Bain's terms.) Lewin answered his question by intuition, of which, as we all admiringly remember, he possessed an abundance. Behavior theory avoids from the outset what is logically a pseudoquestion. For in behavior theory, Krech's image of it notwithstanding, every response depends in principle, that is, as long as we have not empirically ascertained the contrary, upon all variables. Furthermore, some of the intervening variables of behavior theory, such as habit strength, by virtue of their historical nature, represent inseparably past "perceptions" as well as past "volitions." This inseparability is, as it should be, quite independent of the fragile results of this or that style of introspection.

This, as I understand it, is the burden of Krech's argument. The proposition it primarily intends to demonstrate is, I suggest, the dogma of total dynamic interdependence. Four of his comments lend additional support to this interpretation. First, Krech admits that of late many psychologists have tried to break down the walls between the three compartments. Laudable as they may be, these attempts are yet, as he sees it, foredoomed to failure. One of them, he calls it the parliamentary approach of give and take, consists, in the language of this report, in the search for laws in which the variables of the several compartments occur jointly, as those of mechanics and thermodynamics occur in the first principle. To reject, as Krech does, this approach on logical grounds alone amounts to the assertion of some sort of emergence, the rejection of theories which, as all theories in all sciences so far known, contain and work with composition rules. Second, Krech is unduly censorious of the classical Gestaltists for not having been Gestaltish enough when they did not, in their work on perception, manipulate the nonperceptual variables which are now being brought to the fore in the new type of experiment. As I have shown before, this scepticism is quite exaggerated. Third, Krech takes the behavior theorists to task for assuming that two animals, having under different conditions of food deprivation learned the same maze to the same criterion, have "learned the same thing." The only precise meaning I can find for this phrase within the context of behavior theory is that under these conditions certain habit strengths in these two animals have the same value. Now I do not happen to know and, as a logician of science, do not particularly care, whether behavior theory in its latest state actually asserts this,

or whether, if it does, the predictions based on this assertion are borne out in the laboratory. The point is, rather, that whether or not such a prediction is successful is, for the behavior theorist as well as for the positivistic logician, an empirical matter. Krech knows, on logical grounds alone, that it must fail. Nothing could show more clearly that the doctrine of total dynamic interdependence is a dogma held a priori. If there is still any doubt left on this score, Krech himself says, fourth, that upon his principles one "would never assume 'all other things being constant' either in the experimental situations or the resulting 'behaviors' " (21). This is, in scientific phrasing, Hegel's concrete universal, the unique individual in the unique situation which do not really yield to the "abstractive" methods of science. The logical consequence is *verstehende Psychologie* and *geisteswissenschaftliche Methode*. No wonder, then, that the leading continental philosophers of psychology, such as the Frenchman Merleau-Ponty, whom I have decided to exclude from this report because they are all part literary characterologists, part existentially refurbished Hegelians, show great sympathy for Gestalt.

The guarantee, the symbol, or the consummation, I hardly know how to put it, of the sort of unitary theory his holism requires is for Krech the physiological model. This, I suggested before, is the function his hypothetical construct, the dynamic system, has in the economy of his thought. But it is not quite accurate to speak of these "dynamic systems" as physiological models. They are too vague for that by any standard. They are, of course, the physiological correlates of behaviorally defined states; and the changes and processes they undergo are the correlates of behavioral ones. But all we are told beyond this is, in substance, that they are electrochemical in nature; that they are not closed, yet relatively differentiated from each other; that some of them are rather stable; and that they are not too localized and not too diffuse either, to accommodate the various recent and current styles of physiological speculation. Compared with these "constructs," Hebb's speculation sounds almost like a laboratory report and even the models Köhler occasionally proposed are marvels of specificity. To say as much or, rather, as little as Krech says is therefore safe, even at the present stage of physiological psychology. Only, it is hard to see what excess meaning can be wrung from such truisms. The way dynamic systems insure a unitary theory is best explained by means of a comparison. Consider a stone. It has a weight, a shape, a color, a temperature, and so on; and every stone has all these properties all the time. What goes for stones goes for dynamic systems, which are physical objects after all, although very complicated ones; so I shall not exercise my imagination to produce a list of properties all of which every dynamic system possesses all the time. Assume that there is such a list; assume furthermore that all the variables on it totally interact; assume finally that jointly these variables correspond, by whatever correspondence there can be between the entities of a holistic theory and those of an atomistic one, to all the variables of the three fragmentary theories. This, I sug-

gest, is the way in which Krech's "physiology" supports his "theory." At one fell swoop it reveals connections and identities which the three kinds of separatists unhappily prevent themselves from seeing. Consider a hungry rat. That it is hungry means that there is somewhere a tension or disequilibrium in some dynamic system. Again, consider someone irked by a picture hung obliquely. In this case, too, a tension or disequilibrium is set up somewhere in some dynamic system. Yet, for the pagan separatist the one offers a problem in motivation, the other a problem in perception. For the apostle of Gestalt who knows neither Jew nor gentile it is all one, namely, tension or disequilibrium. Thus unification is achieved. The achievement is verbal; its price triviality. In this respect Krech's "physiology" reminds one of Lewin's "topology." Neither gets beyond exploiting the dead and usually unnoticed spatial, kinematic, and kinaesthetic metaphors that lie buried in many words of our every day and scientific language, such as 'direction,' 'rigidity,' 'force,' 'tension,' 'equilibrium.'

Stated roughly, the upshot of the new type of experiment on perception is that two percepts of one and the same object are different if the two perceivers, or the same perceivers on different occasions, are in relevantly different motivational states. This throws some doubt on the similarity which according to the isomorphism doctrine obtains between the object of perception, the percept, and the physiological correlate of the latter; not too disturbingly and not too surprisingly since, as was pointed out before, this "similarity" was always very vague and logically very ill defined. Incidentally and with their characteristic moderation, Bruner & Postman (10) comment on this point. Against them Luchins (26) rises in defense of Gestaltist orthodoxy. Now it seems to me rather obvious and not in need of much defense that one can go on playing, for whatever it is worth, the purely verbal game of isomorphism provided only one includes the subject itself in his environment and his personality in his phenomenal field. Unfortunately, it is not too clear what it means to include in the phenomenal field as rich a notion of Self as this would require. Also, it is a matter of record that, putting it conservatively, the classical documents on isomorphism are not too explicit on such matters. The emphasis there is rather on the object of perception. Accordingly, Luchins' argument is long, heated, and involved. It uses many very general and very ambiguous words, each with several of its meanings, and builds upon this slender basis a dialectical tower that vanishes into the clouds. Bruner in his reply (8) quietly reduces the issue to its size.

An experimental demonstration that, say, size estimates of coins depend on economic need or status is, rather obviously, as relevant to the theory of personality as to that of perception. Since the symposium I mentioned is devoted to the logic of this situation, it is not surprising that a second symposium, on "Theoretical Models and Personality Theory," following almost on the heels of the first and with some of the same participants, does not add much, at least not from the systematic viewpoint. Krech, jointly with Klein (17), repeats what he has in substance said before. Hebb (15) ably defends

theory against such attacks as Skinner's (33); and, in his enthusiasm for physiologizing, makes a historical and psychological case for the *de facto* influence of physiological ideas on all theorizing, including that of the supposedly antipsychological behavior theorists. Systematically his argument is irrelevant. The logic of science is one thing; its history and the psychology of the scientist is another thing. Bruner & Postman (10), in the earlier symposium, say admirably everything that needs to be said about the possibility of dealing adequately within the schema of behavior theory—though not, of course, by means of what now explicitly exists of it—with the whole field of personality. The point is that such concepts as, say, habit family hierarchy and secondary drive provide in principle the means for a behavior-theoretical interpretation of the current notions on personality. Systematically this is all that matters. For, to repeat, the systematic case against Gestaltist and certain psychoanalytic writers is not that what they say about personality is false. Where these writers err, according to their critics, is in the belief that theirs is on logical grounds the only way of saying it. Bruner's & Postman's emphasis is experimental and clinical; Schneider's (32) is systematic and historical. Otherwise, Schneider makes the same points, particularly with respect to traits and the so-called centrality and autonomy of needs, in a critical study of the work of Allport and his fellow students. A paper by Brown & Farber (7) in which they specifically introduce into behavior theory an intervening variable called 'emotion' belongs to psychological theory rather than to theoretical psychology. However, it is of some interest here because it is not merely programmatic; it actually adds to the conceptual apparatus of behavior theory in a direction in which it will have to be developed if it is to make good scientifically the claim which is being made for it logically, that it can, in principle, serve as the articulate nucleus of an articulate and comprehensive theory. There is still another systematic point to this paper. A percept, we saw with Berlyne (6), is in behavior theory a response, not an intervening variable. What a phenomenalist would call an emotion is, on the other hand, a response. It follows that behavior theory must distinguish between its intervening variable, emotion, and the response ordinarily called conscious (perceived) emotion. In doing this it is, by its own logic, led to one of the major Freudian distinctions.

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